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CONDUCTED BY

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"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Polit.* lib. i. cap. 1. Not.

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"Meditationis est perscrutari occulta; contemplationis est admirari perspicua . . . . . Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem."—*Hugo de S. Victore.*

—"Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condat,
Quid toties diros cogat flagrare cometas;
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu."

J. B. Pinelli ad Mazonium.

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ERRATA IN VOL. XXI.

Page 407, line 2, *for* $\tau=247°{\cdot}45$ *read* $g=247°{\cdot}45$.
— line 3, *for* $g=611°{\cdot}28$ *read* $\tau=611°{\cdot}28$.
— line 4, *for* hence $\mu h=\frac{\tau-g}{A}$ *read* hence $\mu h=\frac{\tau-g}{\beta}$.
415, last line of Note II, *for* between L and V *read* between L and m^3.

PLATES.

I. Illustrative of Prof. Magnus's Paper on the Propagation of Heat in Gases.

II. Illustrative of Mr. F. Galton's Paper on Meteorological Charts.

III. Illustrative of Prof. Tyndall's Paper on the Absorption and Radiation of Heat by Gases and Vapours.

IV. Illustrative of Mr. C. Tomlinson's Paper on the Cohesion-Figures of Liquids.

V. Illustrative of MM. Kirchhoff and Bunsen's Paper on Chemical Analysis by Spectrum-observations.

VI. Illustrative of Mr. W. S. Jevons's Paper on the Deficiency of Rain in an elevated Rain-gauge; of Dr. Lamont's Paper on the Form of Magnets; and MM. Kirchhoff and Bunsen's Paper on Chemical Analysis by Spectrum-observations.

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[FOURTH SERIES.]

JULY 1861.

I. *On the Propagation of Heat in Gases.* By G. MAGNUS*.

[With a Plate.]

Conduction of Heat by Gases.

THE cooling of a body, when it takes place *in vacuo*, simply depends on the exchange of heat by radiation between the body and the surrounding envelope. If, however, the space in which the cooling takes place is filled with a gas, an ascending current is formed which accelerates the process. The cooling is likewise promoted by the capacity of the gas to transmit heat (or its diathermancy), as well as by its conductibility, assuming that gases can conduct heat. Dulong and Petit, in enunciating their laws of the loss of heat in their comprehensive memoir *Sur la Mesure des Températures et sur les Lois de la Communication de la Chaleur*, have disregarded the latter actions, manifestly because they could neglect them as being infinitely small in comparison with the influence of the ascending current. Accordingly, since the appearance of their memoir, it has been universally assumed that the differences of cooling in various gases depend on the different mobility of their particles. This was the more justifiable, because almost simultaneously with Dulong and Petit's investigation Sir H. Davy's celebrated memoir† on Flame appeared, in which he says, "It appears that the property of elastic fluids to withdraw caloric from the surface of solid bodies increases as their density decreases, and that there is something in the con-

* Read before the Academy of Sciences of Berlin, July 30, 1860, and February 7, 1861. Translated by Dr. E. Atkinson. [A short abstract of a portion of the results has already appeared in this Journal, vol. xx. p. 510.]

† Phil. Trans. 1817, part. I, p. 61.

Despretz has since shown* that the conduction of heat in water follows the laws given by Fourier for its conduction in metals. A conductibility of heat in gases has never been imagined to exist. Although it is in any case very small, it appeared interesting to investigate what influence it might exert, and whether differences existed in the conductibility of different gases; for the deportment of gases is of especial importance, not only for the laws propounded by Dulong and Petit, but also for any theory of the nature of heat.

The more immediate inducement to this investigation was a repetition of Grove's† interesting experiment, that a platinum wire is less strongly heated by a galvanic current when surrounded by hydrogen than when it is in atmospheric air or any other gas.

On the first publication of these experiments, M. Poggendorff ‡ expressed the opinion that they depended on the laws which Dulong and Petit had established for the cooling of a body heated in the ordinary manner. Clausius § has since shown the concordance between Grove's results and the numbers obtained by Dulong and Petit.

In repeating Grove's experiments, I found that hydrogen exerts its preventive action even when only a very thin layer surrounds the platinum wire. Two very thin, equally long platinum wires were enclosed in tubes of 1 millim. diameter, one of the tubes being filled with atmospheric air and the other with hydrogen. On passing the current through both wires in succession, the one surrounded by atmospheric air became strongly incandescent, while the other did not even reach a red heat. It is scarcely necessary to say that the result was always the same, whichever of the two tubes was filled with hydrogen. Even when the tube filled with hydrogen was quite horizontal, the wire did not become incandescent. As the existence of currents in such a narrow horizontal tube can scarcely be assumed, it appeared improbable that the mobility of the particles of hydrogen was the cause of the strong cooling of the wire.

It is also impossible to conceive why currents produced by differences of temperature should be stronger in hydrogen than in other gases. This gas, it is well known, instead of being more, is even somewhat less expansible than atmospheric air. Hence the same differences in temperature produce in hydrogen less change in the specific gravity than in atmospheric air. But it is by these changes alone that currents are produced in gases. Even if the friction of the particles exercises any influence, and

* *Ann. de Chim.* S. 2. vol. lxxi. p. 206.

† Phil. Mag. vol. xxvii. p. 445; vol. xxxv. p. 114. Pogg. *Ann.* vol. lxxviii. p. 366.

‡ Pogg. *Ann.* vol. lxxi. p. 197. § Ibid. vol. lxxxvii. p. 501.

offers a greater hindrance to motion in other gases than in hydrogen, still this hindrance is in any case so small as not to cancel the influences of the greater expansibility of other gases, such as carbonic acid or sulphurous acid.

But if the expansion in hydrogen can produce no stronger currents than in other gases, there remains no other assumption to explain the more rapid cooling in it than that this gas can conduct heat—that is, can give it from particle to particle, as is the case with metals—and that it possesses this property in a higher degree than other gases. The small density of hydrogen militates against this assumption; and it appeared necessary to decide by a few experiments how far it was correct. Accordingly in a glass tube 2 centims. broad and 10 centims. long, and closed at one end, a thermometer was fitted air-tight, so that the bulb was in the middle of the tube, while the graduation was above the cork. In order to fill the tube with different gases, there were two narrow glass tubes fitted into holes near the thermometer. Outside the tube they were bent at right angles, and could be closed by stopcocks. After water had been boiled in a capacious flask until all air had been expelled, the tube which had been previously filled with gas, was introduced into the flask, during the ebullition, in such a manner that it was entirely surrounded by vapour. The time was then measured which elapsed before the thermometer rose from 20° to 80° C. or 90° C.

The following results were obtained for the time necessary to heat the thermometer in the different gases:—

In	20° to 80°.	20° to 90°.
Atmospheric air .	3·5 minutes.	5·25 minutes.
	3·5 „	5·25 „
	3·5 „	5·2 „
Hydrogen . . .	1·0 „	1·5 „
	1·0 „	1·25 „
	1·1 „	1·4 „
	1·0 „	1·5 „
Carbonic acid . .	4·25 „	6·5 „
	4·25 „	6·25 „
Ammonia . . .	3·5 „	5·5 „
	3·5 „	5·5 „
	3·5 „	5·25 „
	3·5 „	5·25 „

Analogous experiments have been made by Leslie*, Dalton†,

* Inquiry into the Nature of Heat, p. 483.

† Memoirs of the Manchester Literary and Philosophical Society, vol. v. part 2. p. 379.

Davy* and others; but they, like Dulong and Petit, introduced the heated thermometer into a space which was successively filled with different gases, and observed the times which were necessary for the same cooling in these gases. Currents were thereby produced within the gas, which in my experiments were very small if not entirely absent, for the tube containing the gas was heated almost equally on all sides, above as well as below. But as the times which the thermometer required to become heated varied considerably, it appeared probable that the heating in the gases was not produced by currents alone, but that there was also a propagation of heat from particle to particle—in other words, a conduction. Accordingly I next made a series of experiments in which the gases were only heated from above, and observed the temperatures which a thermometer placed in them ultimately assumed. As in this case also the temperature was always higher in hydrogen than in other gases, and was also very different in them, I was confirmed in the conclusion that gases can conduct heat. It might still be objected that in the heating from above currents were formed, which caused the differences in temperature. There was a ready means of testing this objection. For if gases actually can conduct heat, the temperature which a thermometer assumes in a space heated from above, must be lower when the conducting substance is absent, that is, if the space is exhausted. In order to investigate whether this is the case I made use of the following apparatus.

Experiments on Conduction.

On a very thin glass vessel A B, fig. 1, Plate I., 56 millims. wide and 160 millims. in height, a second vessel C was fixed by fusion, of the same diameter, but only 100 millims. in height. A B is provided with a lateral tubulure D, in which a thermometer *f g* is hermetically fitted in such a manner that its bulb is in the axis of A B, and 35 millims. under the bottom of *l*, while the horizontal scale is outside A B. The lower end of A B is closed by means of a cork, in which are two narrow glass tubes provided with stopcocks, which serve to fill A B with different gases. Boiling water was poured into C, and then, from a flask at some distance in which water boiled, steam was passed into this water by the glass tube *p p*, so as to keep it in a state of ebullition. A plug of cotton wool prevented spirting.

In order to compare the thermometer-indications, obtained in using different gases, it was necessary to ensure that the space surrounding the vessel A B was always at the same temperature. For this purpose the vessel A B, with its thermometer,

* Philosophical Transactions for 1817, part 1. p. 60. Schweigger's Journal, xx. p. 154.

was placed in a glass cylinder P Q, 235 millims. wide and 400 millims. in height. This stood in a second similar cylinder X Y, so that there was a space of 30 millims. on every side, which was filled with water. In order that the internal cylinder P Q might not be raised by this water, flat leaden weights were placed on its base, which loaded it so that it rested on the cork supports U U. This cylinder was closed at the top by a hollow metallic cylindrical cover, E E, 75 millims. deep, in which water was poured. In this cover there was a cylindrical aperture $d\,d'\,s\,s'$, 55 millims. in diameter. The vessel C which received the boiling water was fitted in this aperture by means of a slit cork; it was prevented from falling by a couple of metal slides, $s\,s'$, fitted on the under surface of the cover. This arrangement served at the same time to fasten the whole apparatus A B C.

The heat which the vessel radiated laterally, heated the water in the cover E E. In order to keep it at an invariable temperature, cold water continually flowed from a high reservoir through the tube $r\,r$, while the heated water escaped by a siphon $h\,h$. To observe the temperature on the inner cylinder there were several thermometers, one of which, $k\,k$, was placed in a horizontal position right under the cover E E; a second, m, was suspended in the middle of the space, and a third, l, near the bottom. During the experiment all these thermometers were kept at the same temperature, 15° C. For this it was necessary to have the room in which the experiments were performed at about this temperature.

The bulb of the inner thermometer $f\,g$ was protected by a screen $o\,o$ from direct radiation from above. At first I used a cork screen, but afterwards one of silvered copper foil. According to the kind of gas contained in A B, the thermometer reached its highest point and kept it unchanged, in from twenty to forty minutes from the time at which hot water was poured into the vessel C and steam passed into it. In the same kind of gas, the density being constant, the maximum was always reached in the same time; and provided that the thermometer $f\,g$, with its screen, always remained in a fixed position and distance from the vessel C, the temperature did not vary more than 0°·1 to 0°·2 C. Under these conditions also the gradual increase of the temperature, up to the maximum, took place in such a manner that after the same time the thermometer always indicated the same temperature. This concordance furnished a proof of the accuracy of the method.

Before turning to a few of the conclusions from the numbers obtained, it will be convenient to enter upon the circumstances which influence the maxima of temperature.

The heat proceeding from the lower surface of the vessel C is

propagated either by radiation alone, or by radiation and conduction. The thermometer is indeed protected from direct radiation by the cork screen; but this screen itself becomes heated by a long-continued action of the rays, and then gives part of its heat to the thermometer. I confess I at first believed that the heat transferred in this manner to the thermometer would be scarcely perceptible with a screen of 2 millims. thickness, and would in any case be less than with a metal screen. Hence the greater part of the experiments were made with a cork screen. Afterwards, however, I found that a metal screen, although six times as thin as a cork screen, is a better protection against radiation. This doubtless depends upon the fact that a metal screen absorbs fewer of the rays, and also radiates worse than the cork screen; for when the silvered copper foil was blackened on both sides by a tallow candle, the thermometer was heated more than by the cork screen. Hence the metal screen was never used blackened. But whatever the nature of the screen, even when it consisted of two metal plates with an interposed layer of air, the thermometer after the lapse of a sufficient time always attained an invariable temperature, just as it did when without a screen. Other circumstances being the same, this was highest when the thermometer was without a screen. In an apparatus similar to that represented in A B C, Pl. I. fig. 1, but in which the thermometer was somewhat more distant from the vessel of boiling water, the temperatures which it indicated in atmospheric air under a pressure of 1 atmosphere were as follows:—

Cork screen 2 millims. thick.	Two copper foils 1 millim. distant.	No screen.
23° C.	21·5	25·5.

It might be thought that the temperatures obtained in different gases, with the use of different screens, would be proportional to one another, since the different screens would absorb proportional quantities of the heat incident upon them, and would again part with proportional quantities. But the result has shown that, although these temperatures do follow the same series (that is, if in one gas the temperature with the use of one screen is higher than in another, it is also higher when another screen is used), yet that there is no proportion between the two cases. This arises from the fact that, besides the screen, the side of the vessel A B which becomes heated during the experiment, also acts on the thermometer. Although the vessel is surrounded on the outside with air at 15°, it continually receives heat on the inside, partly from the air in contact, partly by radiation from the thermometer *f g*, and partly from the heated base of the vessel C. In consequence of this, the side, although of very thin

glass in the neighbourhood of C, assumes temperatures which are higher than 15° C. Although the thermometer is protected against rays which proceed from the side above, it is directly exposed to the rays which come from those parts of the side which are lower than the screen. And as these parts of the side receive more rays when the vessel A B is filled with a gas which readily transmits heat (that is, with a better-conducting gas) than if it contains a gas which possesses these properties in a lower degree, the influence of the side must change with the kind of gas, and cannot therefore be proportional to the indications of the thermometer.

In the following Tables the temperatures are given which have been obtained by the use of two different screens in different gases, at different densities. In obtaining these results, I have had the advantage of the careful help of M. Rüdorff, who for some time has been engaged in my laboratory. The temperatures are counted from that of the surrounding medium, that is, from 15° C.

Gas.	Pressure. mm	Thermometer with Cork screen.	Thermometer with Metal screen.
Atmospheric air . . .	759·4	9·6 C.	
	753·2		7·0 C.
	741·5	9·5	
	738·0	9·5	
	553·3	9·6	
	373·0	10·0	
	356·0	10·1	
	194·7	11·0	
	15·3	11·5	
	11·6	11·7	
	11·6		7·8
Oxygen	771·2	9·6	
	10·0	11·6	
Hydrogen	760·0	13·0	
	763·5		12·0
	517·7	12·5	
	195·4	12·1	
	11·7	11·8	
	9·6	11·6	
	13·8		8·6
Carbonic acid	750·4	8·2	
	765·3	8·2	
	309·1	9·3	
	16·4	11·3	

Gas	Pressure. mm	Thermometer with Cork screen.	Thermometer with Metal screen
Carbonic oxide . . .	760·0	9·5 C.	
	758·9		6·8 C.
	14·4		7·8
	11·0	11·6	
Protoxide of nitrogen .	760·0	8·8	
	752·5		6·3
	289·0		6·5
	17·7		7·5
	12·0	11·5	
Marsh-gas	771·3	9·4	
	764·2		7·0
	306·8		7·3
	13·3		7·8
	12·0	11·6	
Olefiant gas	749·1	9·0	
	319·2	9·9	
	268·8	10·0	
	19·8	11·7	
Ammonia	770·3	8·1	
	746·5	8·3	
	267·7	9·4	
	63·3	10·8	
	18·7	10·9	
	15·4	11·0	
Cyanogen	760·0	8·8	
	14·0	11·4	
Sulphurous acid . .	757·3	7·8	
	763·3	8·0	
	301·1	9·1	
	11·4	11·0	

The hydrogen used in the experiments was prepared from zinc and sulphuric acid; it was dried by chloride of calcium, but not further purified.

The oxygen was prepared from chlorate of potass and binoxide of manganese.

The carbonic acid was liberated from marble by dilute hydrochloric acid, and then passed through a tube containing bicarbonate of soda.

The carbonic oxide was obtained by heating formiate of soda with sulphuric acid, and

The protoxide of nitrogen was obtained from nitrate of ammonia.

The marsh-gas was obtained from acetate of soda with lime and caustic soda.

The olefiant gas was obtained partly by Wöhler's method, from a mixture of alcohol, sulphuric acid, and sand; and partly by Mitscherlich's method, of passing alcohol vapour with sulphuric acid at 165° C. All these gases were dried by chloride of calcium.

The ammonia was prepared from sal-ammoniac and burnt marble, and dried by passing through a tube of caustic soda.

The sulphurous acid was generated from sulphuric acid and mercury, and dried by chloride of calcium and sulphuric acid.

The cyanogen was prepared from dry cyanide of mercury.

The great concordance between the heating of the thermometer in oxygen and in atmospheric air, shows that this would be the case in nitrogen, and makes a determination in this gas unnecessary.

If the temperatures obtained in the different gases with the use of a screen and under the pressure of an atmosphere are compared, we obtain for

Atmospheric air . .	9·6	or	100·0
Oxygen	9·6	„	100·0
Hydrogen . . .	13·0	„	135·4
Carbonic acid . .	8·2	„	85·4
Carbonic oxide . .	9·5	„	98·9
Protoxide of nitrogen	8·8	„	91·6
Marsh-gas . . .	9·4	„	97·9
Olefiant gas . . .	9·0	„	93·7
Ammonia	8·1	„	84·3
Cyanogen	8·8	„	91·6
Sulphurous acid . .	7·8	„	81·2

The temperatures obtained in these gases when greatly rarefied, are not very concordant, because the small quantity of gas still present doubtless exercises an influence; but if a temperature of $26°{\cdot}7 - 15° = 11°{\cdot}7$ C. be assumed as the most likely for vacuum, and if this be put = 100, the proportion between the temperatures obtained in the other gases under the pressure of an atmosphere, are as—

	Temperature.
Vacuum	100
Atmospheric air . . .	82·0
Oxygen	82·0
Hydrogen	111·1
Carbonic acid	70·0
Carbonic oxide	81·2
Protoxide of nitrogen . .	75·2
Marsh-gas	80·3
Olefiant gas	76·9
Ammonia	69·2
Cyanogen	75·2
Sulphurous acid . .	66·6

It follows from these numbers that hydrogen really conducts heat in a manner similar to the metals; for the temperature which a thermometer placed in it ultimately assumes is higher as the gas is denser.

Of all gases this is only the case with hydrogen, with all the others the temperature is higher when they are more rarefied. It follows therefore that these gases oppose a hindrance to the transmission of radiant heat, and that they are athermanous to such an extent that their athermancy exercises a greater resistance than their capacity to conduct heat. This property is, however, not entirely absent; for, apart from other reasons already adduced by Dalton and Biot, which speak for a conductibility of heat by gases, it would be contrary to all other known laws if we assumed that the capacity of conducting heat was confined to hydrogen. But it is certainly very remarkable that this gas, the lightest of all, possesses the greatest conductibility.

This surprising result has led me to undertake a few experiments with a view of removing, as far as possible, any doubt as to conductibility. For as the upper part of the side of the vessel A B in the neighbourhood of C also became gradually heated, it might be supposed that although the temperature of this side decreases from above downwards, yet that currents are produced in the gases contained in A B, and that the differences of temperature observed only arise from these currents. This assumption is indeed refuted by the fact that, owing to currents, the temperatures in an exhausted space cannot be higher than in one filled with air. In order to remove every objection, I repeated the experiments just mentioned in such a manner that the apparatus A B, or an entirely similar one, was filled with a light substance, with feathers or eider-down, or with cotton wool. It then appeared that the denser the light substance, the higher was the temperature which the thermometer assumed. This higher temperature was therefore certainly not produced by a motion of the air. When the air among the cotton was removed as completely as possible, the thermometer did not attain the same temperature as before, when the interstices were filled with air. The small difference might be caused by an alteration in the density which the cotton had experienced on exhausting the air in the neighbourhood of the thermometer; for the various observations made with the same quantity of cotton gave similar deviations. But when hydrogen was introduced among the cotton, the thermometer always rose higher than when the space was filled with atmospheric air. Hydrogen produced the same effect, whether eider-down or cotton wool was used. The following are a few of the numbers :—

The apparatus contained.	Atmospheric air	Hydrogen	Vacuum.
	under a pressure of 1 atmosphere.		
Loose cotton	7·2° C.*	11·0° C.	7·0° C.
,, ,,	7·7	11·0	
,, ,,	7·2		
,, ,,	7·5	11·0	7·0
,, ,,	7·5		7·1
,, ,,	6·0		
Eider-down	6·0		

After these results it cannot be doubted that hydrogen conducts heat, and that in a higher degree than all other gases. This is the more unexpected, since although the conductibility is not directly dependent on the density of bodies (for example, platinum conducts worse than copper or silver), yet the metals, the densest of all bodies, are the best conductors, and in general the looser and less dense substances conduct worse than the denser ones. If hydrogen exhibits in this respect a deviation, a new proof is afforded of that similarity to the metals, so often maintained from its chemical relations.

[To be continued.]

II. *On a supposed Failure of the Calculus of Variations.* *By* G. B. Airy, *Esq., Astronomer Royal*†.

PROFESSOR JELLETT, in his comprehensive Treatise on the Calculus of Variations, has alluded twice (pages 161 and 365) to the problem "To construct upon a given base A B a curve such that the superficial area of the surface generated by its revolution round A B may be given, and that its solid content may be a maximum." The curve found by Professor Jellett's treatment is a semicircle, and the solid therefore is a sphere. On this he remarks, page 365, "The solution is not given by the sphere, inasmuch as its superficial area is a determinate function of A B [that is, supposing the sphere of the solution to have its diameter equal to and coinciding with A B], and cannot therefore be made equal to any other given quantity." And in page 366 he concludes, "The method therefore fails altogether."

Mr. Todhunter has cited this solution and remark of Professor Jellett, in his invaluable 'History of the Calculus of Variations,' page 410. Mr. Todhunter points out the form of the solution when the solid required has circular ends, but does not allude further to the case considered by Professor Jellett. And thus the matter is left, as an apparent failure of the Calculus.

* The temperatures are counted from ·15° C. upwards.

† Communicated by the Author.

I submit the following solution, as what I believe to be the real interpretation of the formulæ given by the Calculus. It is founded upon these three principles :—

(1) There is nothing to prevent us from accepting as solution of the problem a discontinuous curve, provided the different parts meet in a way which is suitable to the conditions of the problem. Mr. Todhunter in several places has alluded to such discontinuity (see pages 19 and 174).

(2) If, in the solution given immediately by the Calculus of Variations, we are certain that no accidental or adventitious factor has been introduced; and if we find that the solution, expressed under the form $\phi\left(x, y, \frac{dy}{dx}, \text{\&c.}\right)=0$, is the product of two factors, then we are bound to consider each of the curves represented by the two factors as a good and sufficient solution of the problem.

(3) And, to exhibit the solution in its utmost generality, we must use both the solutions given by these curves, in such combination as the circumstances of the problem indicate to be proper.

I now proceed to apply these principles to the problem before us.

Since $\pi\int dx\,.\,y^2$ is to be maximum, while $2\pi\int dx\,.\,y\sqrt{(1+p^2)}$ is given, then if a be a constant to be determined hereafter, the value of V will be

$$y^2+2ay\sqrt{(1+p^2)}\,;$$

and treating this in the usual way, we find

$$\frac{2ay}{\sqrt{(1+p^2)}}=b-y^2,$$

where b is another constant produced by integration. It is certain here that no factor has been introduced.

Since the curve is to meet the axis, $y=0$ at certain points, and $\sqrt{(1+p^2)}$ is never $=0$. Hence b must $=0$; and our equation becomes

$$\frac{2ay}{\sqrt{(1+p^2)}}=-y^2,$$

or

$$y\left\{\frac{2a}{\sqrt{(1+p^2)}}+y\right\}=0\,;$$

which is satisfied by either of the following,

$$\frac{2a}{\sqrt{(1+p^2)}}+y=0,$$

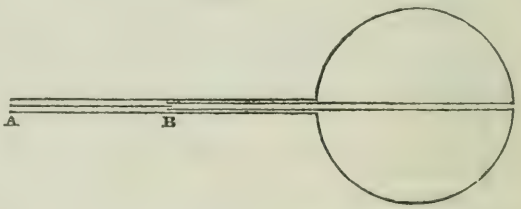
$$y\qquad\qquad=0.$$

The first of these denotes a sphere of radius $-2a$, the first or last limit upon the axis of x being arbitrary. The second denotes a cylinder whose radius is indefinitely small. And the union of the two, which gives the complete solution of the problem, is a sphere of such a radius that its surface has the prescribed value, connected by indefinitely small cylinders or pipes with the points adopted as the limits of x, that is, with A and B.

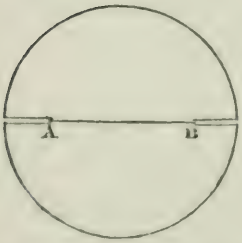
The following diagrams may be conceived to represent the various forms which the solution takes. The first is peculiar to the case when the diameter of the sphere which has the given superficies is less than A B; the second is peculiar to the case when the diameter is greater than A B; the third and fourth both apply to both cases. The diameter of the small pipe is made finite, to be visible to the eye.

Form 1.

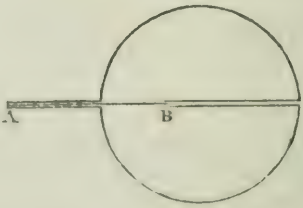
Form 4.



Form 2.



Form 3.



The practical solution evidently is, that a sphere is to be constructed, at any part of the axis of x, whose diameter is such that its surface will be equal to the surface prescribed in the data of the problem. But the metaphysical solution, containing the idea of the tubular connexions with A and B, enables us also to satisfy the condition of terminating the integrations at any points that we may select, not necessarily defined by the position of the sphere.

Royal Observatory, Greenwich,
June 5, 1861.

III. *On the Theory of Types in Chemistry.*

By T. Sterry Hunt, *M.A. F.R.S.**

IN the *Annalen der Chemie und Pharmacie* for March 1860 (vol. cxiii. p. 293), M. Kolbe has given a paper on the natural relations between mineral and organic compounds, considered as a scientific basis for a new classification of the latter. He objects to the four types admitted by Gerhardt, namely, hydrogen, hydrochloric acid, water, and ammonia, that they sustain to organic compounds only artificial and external relations, while he conceives that between these and certain other bodies there are natural relations having reference to the origin of the organic species. Starting from the fact that all the bodies of the carbon series found in the vegetable kingdom are derived from carbonic acid with the concurrence of water, he proceeds to show how all the compounds of carbon, hydrogen, and oxygen may be derived from the type of an oxide of carbon, which is either C^2O^4, C^2O^2, or the hypothetical C^2O.

When in the former we replace one atom of oxygen by one of hydrogen, we have C^2O^3H, or anhydrous formic acid; the replacement of a second equivalent would yield $C^2O^2H^2$, or the unknown formic aldehyde; a third, C^2OH^3, the oxide of methyle; and a fourth, C^2H^4, or formene. By substituting methyle for one or more atoms of hydrogen in the previous formula, we obtain those of the corresponding bodies of the vinic series; and it will be readily seen that by introducing the higher alcoholic radicals, we may derive from C^2O^4 the formulas of all the alcoholic series. A grave objection to this view is, however, found in the fact that, while this compound may be made the type of the aldehydes, acetones, and hydrocarbons, it becomes necessary to assume the hypothetical C^2O^2, HO as the type of the acids and alcohols. Oxide of carbon, C^2O^2, is, according to Kolbe, to be received as the type of hydrocarbons, like olefiant gas (C^2H Me), while C^2O, in which ethyle replaces oxygen, is C^6H^5, or lipyle, the supposed triatomic base of glycerine.

The monobasic organic acids are thus derived from one atom of C^2O^4, while the bibasic acids, like the succinic, are by Kolbe deduced from a double molecule, C^4O^8, and tribasic acids, like the citric, from a triple molecule, C^6O^{12}. He moreover compares sulphuric acid to carbonic acid, and derives from it by substitution the various sulphuric organic compounds. Ammonia, arseniuretted and phosphuretted hydrogen, are regarded as so many types; and by an extension of his view of the replacement of oxygen by electro-positive groups, the ethylides ZnEt, $PbEt^2$,

* Communicated by the Author.

and $BiEt^3$, are by Kolbe assimilated to the oxides of ZnO, PbO^2, and BiO^3.

Ad. Wurtz, in the *Répertoire de Chimie Pure* for October 1860, has given an analysis of Kolbe's memoir (to which, not having the original before me, I am indebted for the preceding sketch), and follows it by a judicious criticism. While Kolbe adopts as types a number of mineral species, including the oxides of carbon, of sulphur and the metals, Wurtz would maintain but three, hydrogen (H^2), water (H^2O^2), and ammonia (NH^3); and these three types, as he endeavoured to show in 1855, represent different degrees of condensation of matter. The molecule of hydrogen, H^2 (M^2), corresponding to four volumes, combines with two volumes of oxygen (O^2) to form four volumes of water, and may thus be regarded as condensed to one-half in its union with oxygen, and derived from a double molecule, M^2M^2. In like manner four volumes of ammonia contain two volumes of nitrogen and six of hydrogen, which, being reduced to one-third, correspond to a triple molecule, M^3M^3, so that these three types and their multiples are reducible to that of hydrogen more or less condensed*.

As regards the rejection of water as a type of organic compounds, and the substitution of carbonic acid, founded upon the consideration that these in nature are derived from C^2O^4, Wurtz has well remarked that water, as the source of hydrogen, is equally essential to their formation, and indeed that the carbonic anhydride, C^2O^4, like all other anhydrous acids, may be regarded as a simple derivative of the water type. Having then adopted the notion of referring a great variety of bodies to a mineral species of simple constitution, water is to be preferred to carbonic anhydride,—first, because we can compare with it many mineral compounds which can with difficulty be compared with carbonic acid; and secondly, because, the two atoms of water being replaceable singly, the mode of derivation of a great number of compounds (acids, alcohols, ethers, &c.) is much more simple and natural than from carbonic acid. As Wurtz happily remarks, Kolbe has so fully adopted the theory of types, that he wishes to multiply them, and even admits condensed types, which are, however, molecules of carbonic acid, and not of water; "he combats the types of Gerhardt, and at the same time counterfeits them."

Thus far we are in accordance with M. Wurtz, who has shown himself one of the ablest and most intelligent expounders of this doctrine of molecular types, as above defined, now almost universally adopted by chemists. He writes, "To my mind this idea of referring to water, taken as a type, a very great

* Wurtz, *Ann. de Chim. et de Phys.* [3.] vol. xliv. p. 304.

number of compounds, is one of the most beautiful conceptions of modern chemistry*;" and again, he declares the idea of regarding both water and ammonia as representatives of the hydrogen type, more or less condensed, to be so simple and so general in its application, that it is worthy "to form the basis of a system of chemistry†."

We have in this theory two important conceptions: the first is that of hydrogen and water regarded as types to which both mineral and organic compounds may be referred; and the second is the notion of condensed and derived types, according to which we not only assume two or three molecules of hydrogen or water as typical forms, but even look on water as the derivative of hydrogen, which is itself the primal type.

As to the history of these ideas, Wurtz remarks that the proposition enunciated by Kolbe, that all organic bodies are derived by substitution from mineral compounds, is not new, but has been known in the science for about ten years. "Williamson was the first who said that alcohol, ether, and acetic acid were comparable to water—organic waters. Hofmann and myself had already compared the compound ammonias to ammonia itself. * * * * * To Gerhardt belongs the merit of generalizing these ideas, of developing them, and supporting them with his beautiful discovery of anhydrous monobasic acids. Although he did not introduce into the science the idea of types, which belongs to M. Dumas, he gave it a new form, which is expressed and essentially reproduced by the proposition of Kolbe. Gerhardt reduced all organic bodies to four types—hydrogen, hydrochloric acid, water, and ammonia‡."

The historical inaccuracies of the above quotation are the more surprising, since in March 1854 I published in the American Journal of Science (vol. xvii. p. 194) a concise account of the progress of these views. This paper was republished in the 'Chemical Gazette' (1854, p. 181), and copies of it were by myself placed in the hands of most of the distinguished chemists of England, France, and Germany. In this paper I have shown that the germ of the idea of mineral types is to be found in an essay of Auguste Laurent§, where he showed that alcohol may be looked upon as water (H^2O^2) in which ethyle replaces one atom of hydrogen, and hydric ether as the result of a complete substitution of the hydrogen by a second atom of ethyle. Hence he observed that while ether is neutral, alcohol is monobasic and the type of the monobasic vinic acids, as water is the type of

* *Répertoire de Chimie Pure*, 1860, p. 359. † Ibid. p. 356.

‡ Ibid. p. 355.

§ "Sur les Combinaisons Azotées," *Ann. de Chim. et de Phys.* November 1846.

bibasic acids. In extending and developing this idea of Laurent's, I insisted in March 1848, and again in January 1850, upon the relation between the alcohols and water as one of homology, water being the first term in the series, and H^2 being in like manner the homologue of acetene and formene, while the bases of Wurtz were said to "sustain to their corresponding alcohols the same relation that ammonia does to water*."

In a notice of his essay, published in September 1848†, I endeavoured to show that Laurent's view might be further extended, so as to include in the type of water "*all those saline combinations* (*acids*) *which contain oxygen*;" and in a paper read before the American Association for the Advancement of Science at Philadelphia, in September 1848, I further suggested that as many neutral oxygenized compounds which do not possess a saline character are derivatives of acids which are referable to the type H^2O^2, "*we may regard all oxygenized bodies as belonging to this type*," which I further showed in the same essay is but a derivative of the primal type H^2, to which I referred all hydrocarbons and their chlorinized derivatives, as also the volatile alkaloids, which were regarded "as amidized species" of the hydrocarbons, in which the residue amidogen, NH^2, replaced an atom of H or Cl, or what is equivalent, the residue NH was substituted for O^2 in the corresponding alcohols‡.§

In the paper published in September 1848, I showed that while water is bibasic, the acids which, like hypochlorous and nitric acids, were derived from it by a simple substitution of Cl and NO^4 for H, were necessarily monobasic; and I then pointed out the possible existence of the nitric anhydride $(NO^4)^2O^2$, which was soon after discovered by Deville. Gerhardt at this time denied the existence of anhydrides of the monobasic acids, while he regarded anhydrides as characteristic of polybasic acids, and indeed was only led to adopt my views by the discovery of the very anhydrides whose formation I had foreseen§.

In explaining the origin of bibasic acids, I described them as produced by the replacement, in a second equivalent of water, of an atom of hydrogen by a monobasic saline group; thus sulphuric acid would be $(S^2HO^6H)O^2$. Tribasic acids, in like manner, are to be regarded as derived from a third equivalent of

* American Journal of Science [2], vol. v. p. 265; vol. ix. p. 65; vol. xiii. p. 206.

† Ibid. vol. vi. p. 173. ‡ Ibid. vol. viii. p. 92.

§ The anhydrides of the monobasic acids correspond to two equivalents of the acid, minus one of water, as $2(C^4H^4O^4) - H^2O^2 = C^8H^6O^6$, while one equivalent of a bibasic acid (itself derived from $2(H^2O^2)$) loses one of water, and becomes an anhydride, as $C^4H^2O^6 - H^2O^2 = C^4O^4$. So that both classes of anhydrides are to be referred to the type of one molecule of water, H^2O^2.

water in which a bibasic residue replaces an atom of hydrogen. The idea of polymeric types was further illustrated in the same paper, where three hydrogen types were proposed, (HH), (H^2H^2), and (H^3H^3), corresponding to the chlorides MCl, MCl^3, and MCl^5. It was also illustrated by sulphur in its ordinary state, which I showed is to be regarded as a triple molecule S^3 (or $S^6=4$ volumes), and referred sulphurous acid SO^2 to this type, to which also probably belongs selenic oxide. (At the same time I suggested that the odorant form of oxygen or ozone was possibly O^3.) Wurtz, in his memoir published in 1855, adopts my view, and makes sulphur vapour at 400° C. the type of the triple molecule. I further suggested* that gaseous nitrogen is NN, an anhydride amide or nitryle, corresponding to nitrite of ammonia, $(NO^3, NH^4O)-H^4O^4=NN$. This view a late writer attributes to Gerhardt, who adopted it from me†. May not nitrogen gas, as I have elsewere suggested, regenerate under certain conditions ammonia and a nitrite, and thus explain not only the frequent formation of ammonia in presence of air and reducing agents, but certain cases of nitrification‡?

I endeavoured still further to show that hydrogen is to be looked upon as the fundamental type, from which the water type is derived by the replacement of an atom of H by the residue HO^2§. In the same way I regarded ammonia as water in which the residue NH replaced O^2.

I have always protested against the view which regards the so-called rational formulæ as expressing in any way the real structure of the bodies which are thus represented. These formulæ are invented to explain a certain class of reactions, and we may construct, from other points of view, other rational formulæ which are equally admissible. As I have elsewhere said, "the various hypotheses of copulates and radicals are based upon the notion of dualism, which has no other foundation than the observed order of generation, and can have no place in a theory of

* American Journal of Science, vol. v. p. 408; vol. vi. p. 172.

† *Ann. de Chim. et de Phys.* vol. lx. p. 381.

‡ The formation of a nitrite in the experiments of Cloez appears to be independent of the presence of ammonia, and to require only the elements of air and water (*Comptes Rendus*, vol. lxi. p. 135). Some experiments now in progress lead me to conclude that the appearance of a nitrite in the various processes for ozone is due to the power of nascent oxygen to destroy by oxidation the ammonia generated by the action of water on nitrogen, the nitrous nitryle; so that the odour and many of the reactions assigned to ozone or nascent oxygen are really due to the nitrous acid which is set free when the former encounters nitrogen and moisture. On the other hand, nascent hydrogen, which readily reduces nitrates and nitrites to ammonia, by destroying the regenerated nitrite of the nitryle, produces ammonia in many cases from atmospheric nitrogen.

§ American Journal, vol. viii. p. 93.

science." All chemical changes are reducible to union (identification), and division (differentiation). When in these changes only one species is concerned, we designate the process as metamorphosis, which is either by condensation or by expansion (homogeneous differentiation). In metagenesis, on the contrary, unlike species may unite, and by a subsequent heterogeneous differentiation give rise to new species, constituting what is called double decomposition, the results of which, differently interpreted, have given origin to the hypothesis of radicals and the notion of substitution by residues, to express the relations between the parent bodies and their progeny. The chemical history of bodies is then a record of their changes; it is, in fact, their genealogy; and in making use of typical formulæ to indicate the derivation of chemical species, we should endeavour to show the ordinary modes of their generation*.

Keeping this principle in mind, let us now examine the theory of the formation of acids. As we have just seen, I taught in 1848 that the monobasic, bibasic, and tribasic acids are derived respectively from one, two, and three molecules of water, H^2O^2. M. Wurtz, seven years later (in 1855), put forth a similar view. He supposes a monatomic radical $PO^{4'}$, a diatomic radical $PO^{3''}$, and a triatomic radical $PO^{2'''}$, replacing respectively one, two, and three atoms of hydrogen in H^2O^2, H^4O^4, and H^6O^6, thus $(PO^{4'}H)O^2$, $(PO^{3''}H^2)O^4$, and $(PO^{2'''}H^3)O^6$. These radicals evidently correspond to PO^5 which has lost one, two, and three atoms of oxygen in reacting upon the hydrogen of the water type; and these acids may be accordingly represented as formed by the substitution of the residue PO^5-O for H, &c.

To this manner of representing the generation of polybasic acids we object that it encumbers the science with numerous hypothetical radicals, and that it moreover fails to show the actual successive generation of the series of acids in question.

When phosphoric anhydride, $P^2O^{10}=(PO^4)^2O^2$, is placed in contact with water, it combines with one equivalent, H^2O^2. The union is followed by homogeneous differentiation, and two equivalents of metaphosphoric acid result;

$$(PO^4)^2O^2+H^2O^2=2(PO^4H)O^2.$$

Two equivalents of this acid with one of water at ordinary temperatures are slowly transformed into two of pyrophosphoric acid by a reaction precisely similar to the last,

$$2(PHO^6)[=(PHO^5)^2O^2]+H^2O^2=2(PHO^5H)O^2;$$

* See "On the Theory of Chemical Changes," Amer. Journ. of Science, vol. xx. p. 226; Lond. Edinb. and Dub. Phil. Mag. [4] vol. v. p. 526; and *Chem. Centralblatt*, 1853, p. 849. Also, "Thoughts on Solution," Amer. Journ. of Science, vol. xix. p. 100; and 'Chemical Gazette,' 1855, p. 92.

and two equivalents of pyrophosphoric acid, when heated with a third equivalent of water, yield in like manner two of tribasic phosphoric acid,

$$2(PH^2O^7) = [(PH^2O^6)^2O^2] + H^2O^2 = 2(PH^2O^6H)O^2 = 2PH^3O^8.$$

Gerhardt long since maintained that we cannot distinguish between polybasic salts and what are called subsalts, which are as truly neutral salts of a particular type. Thus the bibasic and tribasic phosphates are to be looked upon as subsalts which sustain the same relation to the monobasic phosphates that the basic nitrates bear to the neutral nitrates. He succeeded in preparing two crystalline subnitrates of lead and copper, having the formulæ NO^5, M^2O^2, HO (tribasic), and NO^5, M^4O^4, H^3O^3 (quadri or heptabasic), both of which retain their water of composition at 392° F. The compounds of sulphuric acid are,—1st. the true monobasic sulphate, S^2O^6MO, corresponding to the Nordhausen acid and the anhydrous bisulphates; 2nd. the ordinary neutral sulphates, S^2O^6, M^2O^2; 3rd. the so-called disulphates, S^2O^6, M^4O^4, corresponding to the glacial acid density 1·780; 4th. the type S^2O^6, M^6O^6, represented by turpeth mineral; and 5th. the so-called quadribasic sulphates, $S^2O^6M^8O^8$. The copper salt of this type, according to Gerhardt, retains, moreover, 6HO at 392° F.*

Without counting the still more basic sulphates of zinc and copper, described by Kane and Schindler, we have the following salts, which, in accordance with Wurtz's notation, correspond to the annexed radicals:—

1. Unibasic	S^2HO^7	$=S^2O^5$	monatomic.
2. Bibasic	$S^2H^2O^8$	$=S^2O^4$	diatomic.
3. Quadribasic . . .	$S^2H^4O^{10}$	$=S^2O^2$	tetratomic.
4. Sexbasic	$S^2H^6O^{12}$	$=S^2$	hexatomic.
5. Octobasic	$S^2H^8O^{14}$	$=S^2-O^2$	octatomic.

It is easy to apply a similar *reductio ad absurdum* to the radical theory in the case of the oxychlorides and other basic salts, and to show that the radicals of the dualists are often merely algebraic expressions. (See further my remarks in the American Journal of Science, vol. vii. pp. 402–404†.)

The above, which we conceive to be a simple statement of the

* Gerhardt "On Salts," *Journ. de Pharm.* 1848, vol. xii. American Journal of Science, vol. vi. p. 337.

† Those who are familiar with chemical literature will remember an amusing *jeu d'esprit* of Laurent's, in which he invited the attention of the advocates of the radical theory to a newly invented electro-negative radical, *Eurhizene* (*Comptes Rendus des Travaux de Chimie* for 1850, pp. 251 and 376). We observe a late writer in the 'Chemical News' (vol. i. p. 326) proposing, as a new electro-negative radical, under the name of hydrine, the peroxide of hydrogen, HO, the eurhizene of Laurent!

process as it takes place in nature, dispenses alike with hypothetical radicals and residues, both of which are, however, convenient for the purposes of notation. In the selection of a typical form, to which a great number of species may be referred, hydrogen or water merits the preference from its simplicity, and from the important part which it plays in the generation of species. Water and carbonic anhydride are both so directly concerned in the generation of the bodies in the carbon series, that either may be assumed as the type; but we prefer to regard C^2O^4, like the other anhydrides, as only a derivative of the type of water, and eventually of the hydrogen type.

These views were first put forward by myself in 1848, when I expressed the opinion that they were destined to form "the basis of a true natural system of chemical classification;" and it was only after having opposed them for four years to those of Gerhardt, that this chemist, in June 1852, renounced his views, and without any acknowledgment adopted my own*. Already in 1851, Williamson, in a paper read before the British Association, had developed the ideas on the water type to which Wurtz refers above; and to him the English editor of Gmelin's 'Handbook' ascribes the theory. The notion of condensed types, and of H^2 as the primal type, was not, so far as I am aware, brought forward by either of these, and remained unnoticed until resuscitated by Wurtz in 1855, seven years after I had first announced it, and one year after my reclamation, published in the American Journal of Science, in March 1854.

My claims have not, however, been overlooked by Dr Wolcott Gibbs. In an essay on the polyacid bases, he remarks that in a previous paper he had attributed the theory of water types to Gerhardt and Williamson, and adds, "In this I find I have not done justice to Mr. T. Sterry Hunt, to whom is exclusively due the credit of having first applied the theory to the so-called oxygen acids and to the anhydrides, and in whose earlier papers may be found the germs of most of the ideas on classification usually attributed to Gerhardt and his disciples†." It will be seen, from what precedes, that I not only applied the theory, as Dr. Gibbs remarks, but, except so far as Laurent's suggestion goes, invented it and published it in all its details some years before it was accepted by a single chemist.

In conclusion, I have only to ask that future historians will do justice to the memory of Auguste Laurent, and will ascribe to whom it is due the credit of having given to the science a theory which has exercised such an important influence on modern che-

* *Ann. de Chim. et de Phys.* [3] vol. xxxvii. p. 285.

† Proceedings of the American Association, Baltimore, May 1858, p. 197.

mical speculation and research, remembering that my own publications on the subject, which cover the whole ground, were some years earlier than those of Williamson, Gerhardt, Wurtz, or Kolbe.

Montreal, January 1861.

IV. *On the Reduction of Observations of Underground Temperature; with Application to* Professor Forbes's *Edinburgh Observations, and the continued Calton Hill Series. By* Professor William Thomson, F.R.S.*

I. *Analysis of Periodic Variations.*

1. EVERY purely periodical function is, as is well known, expressible by means of a series of constant coefficients multiplying sines and cosines of the independent variable with a constant factor and its multiples. This important truth was arrived at by an admirable piece of mathematical analysis, called for by Daniel Bernoulli, partially given by La Grange, and perfected by Fourier.

2. To simplify my references to the mathematical propositions of this theory, I shall commence by laying down the following definitions:—

Def. 1. A simple harmonic function is a function which varies as the sine or cosine of the independent variable, or of an angle varying in simple proportion with the independent variable. The harmonic curve is the well-known name applied to the graphic representation, on the ordinary Cartesian system, of what I am now defining as a simple harmonic function. It is the form of a string vibrating in such a manner as to give the simplest and smoothest possible character of sound; and, in this case, the displacement of each particle of the string is a harmonic function of the time, besides being a harmonic function of the distance of its position of equilibrium from either end of the string. The sound in this case may be called a perfect unison.

Def. 2. The argument of a simple harmonic function is the angle to the sine or cosine of which it is proportional.

Cor. The argument of a harmonic function is equal to the independent variable multiplied by a constant factor, with a constant added; that is to say, it may be any linear function of the independent variable.

Def. 3. When time is the independent variable, the epoch is

* From the Transactions of the Royal Society of Edinburgh, vol. xxii. part 2. Communicated by the Author.

the interval which elapses from the era of reckoning till the function first acquires a maximum value. The augmentation of argument corresponding to that interval will be called "the epoch in angular measure," or simply "the epoch" when no ambiguity can exist as to what is meant.

Def. 4. The period of a simple harmonic function is the augmentation which the independent variable must receive to increase the argument by a circumference.

Cor. If c denote the coefficient of the independent variable in the argument, the period is equal to $\frac{2\pi}{c}$. Thus if T denote the period, ϵ the epoch in angular measure, and t the independent variable, the argument proper for a cosine is

$$\frac{2\pi t}{\mathrm{T}} - \epsilon;$$

and the argument for a sine,

$$\frac{2\pi t}{\mathrm{T}} - \epsilon + \frac{\pi}{2}.$$

3. *Composition and Resolution of Simple Harmonic Functions of one Period.*

Prop. The sum of any two simple harmonic functions of one period is equal to one simple harmonic function whose amplitude is the diagonal of a parallelogram described upon lines drawn from one point to lengths equal to the amplitudes of the given functions, at angles measured from a fixed line of reference equal to their epochs, and whose epoch is the inclination of the same diagonal to the same line of reference.

Cor. 1. If A, A' be the amplitudes of two simple harmonic functions of equal period, and ϵ, ϵ' their epochs, that is to say, if $A\cos(mt-\epsilon)$, $A'\cos(mt-\epsilon')$ be two simple harmonic functions, the one simple harmonic function equal to their sum has for its amplitude and its epoch the following values respectively:—

$$\text{(amplitude)} \quad \{(A\cos\epsilon + A'\cos\epsilon')^2 + (A\sin\epsilon + A'\sin\epsilon')^2\}^{\frac{1}{2}},$$

$$\text{or } \{A^2 + 2AA'\cos(\epsilon'-\epsilon) + A'^2\}^{\frac{1}{2}};$$

$$\text{(epoch)} \quad \tan^{-1}\frac{A\sin\epsilon + A'\sin\epsilon'}{A\cos\epsilon + A'\cos\epsilon'}$$

Cor. 2. Any number of simple harmonic functions, of equal period, added together, are equivalent to a single harmonic func-

tion of which the amplitude and epoch are derived from the amplitude and epochs of the given functions, in the same manner as the magnitude and inclination to a fixed line of reference, of the resultant of any number of forces in one plane, are derived from the magnitudes and the inclinations to the same line of reference of the given forces.

Cor. 3. The physical principle of the superposition of sounds being admitted, any number of simple unisons of one period co-existing, produce one simple unison of the same period, of which the intensity (measured by the square of the amplitude) and the epoch are determined in the manner just specified.

Cor. 4. The sum of any number of simple harmonic functions of one period vanishes for every argument, if it vanishes for any two arguments not differing by a semicircumference, or by some multiple of a semicircumference.

Cor. 5. The co-existence of perfect unisons may constitute perfect silence.

Cor. 6. A simple harmonic function of any epoch may be resolved into the sum of two whose epochs are respectively zero and a quarter period, and whose amplitudes are respectively equal to the value of the given function for the arguments zero and a quarter period respectively.

4. *Complex Harmonic Functions.*—Harmonic functions of different periods added can never produce a simple harmonic function. If their periods are commensurable, their sum may be called a complex harmonic function.

Cor. A complex harmonic function is the proper expression for a perfect harmony in music.

5. *Expressibility of Arbitrary Functions by Trigonometrical series.*

Prop. A complex harmonic function, with a constant term added, is the proper expression, in mathematical language, for any arbitrary periodic function.

6. *Investigation of the Trigonometrical Series expressing an Arbitrary Function.*—Any arbitrary periodic function whatever being given, the amplitudes and epochs of the terms of a complex harmonic function, which shall be equal to it for every value of the independent variable, may be investigated by the "method of indeterminate coefficients," applied to determine an infinite number of coefficients from an infinite number of equations of condition, by the assistance of the integral calculus as follows:—

Let $F(t)$ denote the function, and T its period. We must suppose the value of $F(t)$ known for every value of t, from $t=o$ to $t=T$. Let M_0 denote the constant term, and let M_1, M_2, M_3, &c. denote the amplitudes, and ϵ_1, ϵ_2, ϵ_3, &c. the epochs of the

successive terms of the complex harmonic functions by which it is to be expressed; that is to say, let these constants be such that

$$(\mathrm{F}t)=\mathrm{M}_0+\mathrm{M}_1\cos\left(\frac{2\pi t}{\mathrm{T}}-\epsilon_1\right)+\mathrm{M}_2\cos\left(\frac{4\pi t}{\mathrm{T}}-\epsilon_2\right)$$
$$+\mathrm{M}_3\cos\left(\frac{6\pi t}{\mathrm{T}}-\epsilon_3\right)+\&\mathrm{c}.$$

Then, expanding each cosine by the ordinary formula, and assuming

$$\mathrm{M}_1\cos\epsilon_1=\mathrm{A}_1,\quad \mathrm{M}_2\cos\epsilon_2=\mathrm{A}_2,\ \&\mathrm{c}.,$$
$$\mathrm{M}_1\sin\epsilon_1=\mathrm{B}_1,\quad \mathrm{M}_2\sin\epsilon_2=\mathrm{B}_2,\ \&\mathrm{c}.,$$

we have

$$\mathrm{F}(t)=\mathrm{A}_0+\mathrm{A}_1\cos\frac{2\pi t}{\mathrm{T}}+\mathrm{A}_2\cos\frac{4\pi t}{\mathrm{T}}+\mathrm{A}_3\cos\frac{6\pi t}{\mathrm{T}}+\&\mathrm{c}.,$$
$$+\mathrm{B}_1\sin\frac{2\pi t}{\mathrm{T}}+\mathrm{B}_2\sin\frac{4\pi t}{\mathrm{T}}+\mathrm{B}_3\sin\frac{6\pi t}{\mathrm{T}}+\&\mathrm{c}.$$

Multiplying each member by $\cos\frac{2i\pi t}{\mathrm{T}}\,dt$, where i denotes o or any integer, and integrating from $t=o$ to $t=\mathrm{T}$, we have

$$\int_o^{\mathrm{T}}\mathrm{F}(t)\cos\frac{2i\pi t}{\mathrm{T}}\,dt=\mathrm{A}_i\int_o^{\mathrm{T}}\left(\cos\frac{2i\pi t}{\mathrm{T}}\right)^2dt,$$
$$=\mathrm{A}_i\times\tfrac{1}{2}\mathrm{T},\text{ when } i \text{ is any integer;}$$

or

$$=\mathrm{A}_0\times\mathrm{T},\text{ when } i=0.$$

Hence

$$\mathrm{A}_0=\frac{1}{\mathrm{T}}\int_o^{\mathrm{T}}\mathrm{F}(t)\,dt,$$
$$\mathrm{A}_i=\frac{2}{\mathrm{T}}\int_o^{\mathrm{T}}\mathrm{F}(t)\cos\frac{2i\pi t}{\mathrm{T}}\,dt;$$

and similarly we find

$$\mathrm{B}=\frac{2}{\mathrm{T}}\int_o^{\mathrm{T}}\mathrm{F}(t)\sin\frac{2i\pi t}{\mathrm{T}}\,dt:$$

equations by which the coefficients in the double series of sines and cosines are expressed in terms of the values of the function supposed known from $t=o$ to $t=\mathrm{T}$. The amplitudes and epochs of the single harmonic terms of the chief period and its submultiples are calculated from them, according to the follow-

ing formula:—

$$\tan \epsilon_i = \frac{B}{A_i}; \quad M_i = (A_i^2 + B_i^2)^{\frac{1}{2}}$$

(or for logarithmic calculation,

$$M_i = A_i \sec \epsilon_i).$$

The preceding investigation is sufficient as a solution of the problem, to find a complex harmonic function expressing a given arbitrary periodic function, when once we are assured that the problem is possible; and when we have this assurance, it proves that the resolution is *determinate*, that is to say, that no other complex harmonic function than the one we have found can satisfy the conditions. For a thorough and most interesting analysis of the subject, supplying all that is wanting to complete the investigation, and giving admirable views of the problem from all sides, the reader is referred to Fourier's delightful treatise. A concise and perfect synthetical investigation of the harmonic expression of an arbitrary periodic function is to be found in Poisson's *Théorie Mathématique de la Chaleur*, chap. vii.

II. *Periodic Variations of Terrestrial Temperature.*

7. If the whole surface of the earth were at each instant of uniform temperature, and if this temperature were made to vary as a perfectly periodic function of the time, the temperature at any internal point must ultimately come to vary also as a periodic function of the time, with the same period, whatever may have been the initial distribution of temperature throughout the whole. Fourier's principles show how the periodic variation of internal temperature is to be conceived as following, with diminished amplitude and retarded phase, from the varying temperature at the surface supposed given: and by his formulæ the precise law according to which the amplitude would diminish and the phase would be retarded, for points more and more remote from the surface, if the figure were truly spherical and the substance homogeneous, is determined.

8. The largest application of this theory to the earth as a whole is to the analysis of imaginable secular changes of temperature, with at least thousands of millions of years for a period. In such an application, it would be necessary to take into account the spherical figure of the earth as a whole. Periodic variations at the surface with any period less than a million* of years will,

* A periodic variation of external temperature of one million years' period would give variations of temperature within the earth sensible to one thousand times greater depths than a similar variation of one year's period. Now the ordinary annual variation is reduced to $\frac{1}{70}$th of its superficial

at points below the surface, give rise to variations of temperature not appreciably influenced by the general curvature, and sensibly agreeing with what would be produced if the surface were an infinite plane, except insofar as they are modified by superficial irregularities. Hence Fourier's formulæ for an infinite solid, bounded on one side by an infinite plane, of which the temperature is made to vary arbitrarily, contain the proper analysis for diurnal or annual variations of terrestrial temperature, unless a theory of the effect of inequalities of surface (upon which no investigator has yet ventured) is aimed at.

9. The effect of diurnal variations of temperature becomes insensible at so small a distance below the surface, that in most localities irregularities of soil and drainage must prevent any very satisfactory theoretical treatment of their inward progression and extinction from being carried out. At depths exceeding three feet below the surface, all periodic effects of daily variations of temperature become insensible in most soils, and the observable changes are those due to a daily average, varying from day to day. If now the annual variation of temperature were truly periodic, a complex harmonic function could be determined to represent for all time the temperature at three feet or any greater depth. But in reality the annual variation is very far from recurring in a perfectly periodic manner, since there are both great differences in the annual average temperatures, and never-ceasing irregularities in the progress of the variation within each year. A full theory of the consequent variations of temperature propagated downwards, must include the consideration of non-periodic changes; but the most convenient first step is that which I propose to take in the present communication, in which the average annual variations for groups of years will be discussed according to the laws to which periodic variations are subject.

10. The method which Fourier has given for treating this and other similar problems is founded on the principle of the independent superposition of thermal conductions. This principle holds rigorously in nature, except insofar as the conductivity or

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amount at a depth of 25 French feet, and is scarcely sensible at a depth of 50 French feet (being there reduced, in such rock as that of Calton Hill, to $\frac{1}{1000}$). Hence, at a depth of 50,000 French feet, or about ten English miles, a variation having one million years for its period would be reduced to $\frac{1}{1000}$. If the period were ten thousand million years, the variation would similarly be reduced to $\frac{1}{1000}$ at 1000 miles' depth, and would be to some appreciable extent affected by the spherical figure of the whole earth, although to only a very small extent, since there would be comparatively but very little change of temperature (less than $\frac{1}{20}$ of the superficial amount) beyond the first layer of 500 miles' thickness.

the specific heat of the conducting substance may vary with the changes of temperature to which it is subjected; and it may be accepted with very great confidence in the case with which we are now concerned, as it is not at all probable that either the conductivity or the specific heat of the rock or soil can vary at all sensibly under the influence of the greatest changes of temperature experienced in their natural circumstances; and, indeed, the only cause we can conceive as giving rise to sensible change in these physical qualities is the unequal percolation of water, which we may safely assume to be confined in ordinary localities to depths of less than three feet below the surface. The particular mode of treatment which I propose to apply to the present subject consists in expressing the temperature at any depth as a complex harmonic function of the time, and considering each term of this function separately, according to Fourier's formulæ for the case of a simple harmonic variation of temperature, propagated inwards from the surface. The laws expressed by these formulæ may be stated in general terms as follows.

11. *Fourier's Solution stated**.—If the temperature at any point of an infinite plane, in a solid extending infinitely in all directions, be subjected to a simple harmonic variation, the temperature throughout the solid on each side of this plane will follow everywhere according to the simple harmonic law, with epochs retarded equally, and with amplitudes diminished in a constant proportion for equal augmentations of distance. The retardation of epoch expressed in circular measure (arc divided by radius) is equal to the diminution of the Napierian logarithm of the amplitude; and the amount of each per unit of distance is equal to $\sqrt{\frac{\pi c}{Tk}}$, if c denote the capacity for heat of a unit bulk of the substance, and k its conductivity †.

12. Hence, if the complex harmonic functions expressing the varying temperature at two different depths be determined, and each term of the first be compared with the corresponding term of the second, the value of $\sqrt{\frac{\pi c}{Tk}}$ may be determined either by dividing the difference of the Napierian logarithms of the amplitudes, or the difference of the epochs by the distance between the points. The comparison of each term in the one series with the

* For the mathematical demonstration of this solution, see Note appended to Professor Everett's paper, which follows the present article in the Transactions.

† That is to say, the quantity of heat conducted per unit of time across a unit area of a plate of unit thickness, with its two surfaces permanently maintained at temperatures differing by unity.

corresponding term in the other series gives us, therefore, two determinations of the value of $\sqrt{\frac{\pi c}{k'}}$ which should agree perfectly, if (1) the data were perfectly accurate, if (2) the isothermal surfaces throughout were parallel planes, and if (3) the specific heat and conductivity of the soil were everywhere and always constant.

As these conditions are not strictly fulfilled in any natural application, the first thing to be done in working out the theory is to test how far the different determinations agree, and to judge accordingly of the applicability of the theory in the circumstances. If the test thus afforded prove satisfactory, the value of the conductivity in absolute measure may be deduced from the result with the aid of a separate experimental determination of the specific heat.

13. The method thus described differs from that followed by Professor Forbes, in substituting the separate consideration of separate terms of the complex harmonic function for the examination of the whole variation unanalysed, which he conducted according to the plan laid down by Poisson.

This plan consists in using the formulæ for a simple harmonic variation, as approximately applicable to the actual variation. At great depths the amplitudes of the second and higher terms of the complex harmonic function become so much reduced as not sensibly to influence the variation, which is consequently there expressed with sufficient accuracy by a single harmonic term of yearly period; but at even the greatest depths for which continuous observations have actually been made, the second (or semi-annual) term has a very sensible influence, and the third and fourth terms are by no means without effect on the variations at three feet and six feet from the surface. A close agreement with theory is therefore not to be expected, until the method of analysis which I now propose is applied. It may be added that in the theoretical reductions hitherto made, either by Professor Forbes or others, the amplitudes of the variations for the different depths have alone been compared, and the very interesting conclusion of theory, as to the relation between the absolute amount of retardation of phase and the diminution of amplitude for any increase of depth, has remained untested.

14. In Professor Forbes's paper *, the very difficult operations which he had performed for effecting the construction and the sinking of the thermometers, and the determination of the cor-

* "Account of some Experiments on the Temperature of the Earth at different Depths and in different Soils near Edinburgh." Transactions of the Royal Society of Edinburgh, vol. xvi. part 2. Edinburgh, 1846.

rections to be applied to obtain the true temperatures of the earth at the different depths from the readings of the scales graduated on their stems protruding above the surface, are fully described. The results of five years' observations—1837 to 1842—are given, along with most interesting graphical representations and illustrations. A process of graphic interpolation, for estimating the temperatures at times intermediate between those of the observations, is applied for the purpose of obtaining data from which the complex harmonic functions expressing the temperatures actually observed for the different depths are determined. I am thus indebted to Professor Forbes for the mode of procedure (described below) which I have myself followed in expressing the variations of temperature during the succeeding thirteen years for the Calton Hill station (where alone the observations were continued). The only variation from his process which I have made is, that, instead of taking twelve points of division for the yearly period, I have taken thirty-two, with a view to obtaining a more perfect representation of all the features of the observed variations, and a more exact average for the principal terms, especially the annual and the semi-annual terms of the complex harmonic function expressing them.

15. *Application of the General Theory to Five Years' Observations*—1837 *to* 1842—*at* Professor Forbes's *three Thermometric Stations.*—The first application which I made of the analytical theory explained above, was to the harmonic terms which Professor Forbes had found for expressing the average annual progressions of temperature during the five years' term of observations at the three stations. These terms (which I have recalculated to get their values true to a greater number of significant figures), with alterations of notation which I have found convenient for the analytical expressions, are as follows :—

Three Feet below Surface.

Observatory . . . $45{\cdot}49+7{\cdot}39\cos 2\pi(t-{\cdot}63\ )+0{\cdot}362\cos 2\pi(2t-{\cdot}669)$
Experimental Gardens. $46{\cdot}13+9{\cdot}00\cos 2\pi(t-{\cdot}616)+0{\cdot}737\cos 2\pi(2t-{\cdot}183)$
Craigleith . . . . $45{\cdot}88+8{\cdot}16\cos 2\pi(t-{\cdot}617)+0{\cdot}284\cos 2\pi(2t-{\cdot}154)$

Six Feet below Surface.

Observatory . . . $45{\cdot}86+5{\cdot}06\cos 2\pi(t-{\cdot}686)+0{\cdot}433\cos 2\pi(2t-{\cdot}731)$
Experimental Gardens. $46{\cdot}42+6{\cdot}66\cos 2\pi(t-{\cdot}665)+0{\cdot}501\cos 2\pi(2t-{\cdot}182)$
Craigleith . . . . $45{\cdot}92+6{\cdot}16\cos 2\pi(t-{\cdot}649)+0{\cdot}368\cos 2\pi(2t-{\cdot}305)$

Twelve Feet below Surface.

Observatory . . . $46{\cdot}36+2{\cdot}44\cos 2\pi(t-{\cdot}799)+0{\cdot}075\cos 2\pi(2t-{\cdot}833)$
Experimental Gardens $46{\cdot}76+3{\cdot}38\cos 2\pi(t-{\cdot}782)+0{\cdot}230\cos 2\pi(2t-{\cdot}390)$
Craigleith . . . . $45{\cdot}92+4{\cdot}22\cos 2\pi(t-{\cdot}713)+0{\cdot}067\cos 2\pi(2t-{\cdot}819)$

Twenty-four Feet below Surface.

Observatory . . . $46{\cdot}87+0{\cdot}655\cos 2\pi(t-1{\cdot}013)$
Experimental Gardens $47{\cdot}09+0{\cdot}920\cos 2\pi(t-{\cdot}986)$
Craigleith . . . . $46{\cdot}07+1{\cdot}940\cos 2\pi(t-{\cdot}849)$

The semi-annual terms in these equations present so great irregularities (those for the Calton Hill station, for instance, showing a greater amplitude at 6 feet depth than at 3 feet), that no satisfactory result can be obtained by including them in the theoretical discussion on which we are now about to enter. We shall see later, however, that when an average for the whole period of eighteen years for the Calton Hill station is taken, the semi-annual terms are, for the 3 feet and 6 feet depths, in fair agreement with theory; and for the two greater depths are as small as is necessary for the verification of the theory, and so small as not to be much influenced by errors of observation and of reduction, or of "corrections" for temperature of the thermometer tubes. For the present, we attend exclusively to the annual terms. The amplitudes and epochs of these terms, extracted from the preceding equations, are shown in the following Table :—

Table I. Annual Harmonic Variations of Temperature.

Depths below surface in French feet.	Calton Hill.			Experimental Garden.			Craigleith Quarry.		
	Amplitudes in degrees Fahr.	Epochs of maximum.		Amplitudes in degrees Fahr.	Epochs of maximum.		Amplitudes in degrees Fahr.	Epochs of maximum.	
		In degrees and minutes.	In months and days.		In degrees and minutes.	In months and days.		In degrees and minutes.	In months and days.
Feet.	°	° ′		°	° ′		°	° ′	
3	7·386	226 52	Aug. 19	9·063	221 40	Aug. 13	8·069	220 0	Aug. 14
6	5·063	247 5	Sept. 8	6·661	239 20	31	6·148	233 43	26
12	2·455	287 30	Oct. 19	3·408	281 27	Oct. 13	4·216	256 42	Sept. 17
24	0·655	365 6	Jan. 6	0·920	355 0	Dec. 27	1·836	305 46	Nov. 7

By taking the differences of the Napierian logarithms of the amplitudes, and the differences of epochs reduced to circular measure (arc divided by radius), thus shown for the different depths, and dividing each by the corresponding difference of depths, we find the following numbers :—

TABLE II.—Rates of Logarithmic Diminution in Amplitude, and of Retardation in Epoch, of Annual Harmonic Variations Downwards.

Depths below surface in French feet.	Calton Hill.		Experimental Garden.		Craigleith Quarry.	
	Rate of diminution of Napierian logarithm of amplitude per foot of descent.	Rate of retardation of epoch in circular measure per foot of descent.	Rate of diminution of Napierian logarithm of amplitude per foot of descent.	Rate of retardation of epoch in circular measure per foot of descent.	Rate of diminution of Napierian logarithm of amplitude per foot of descent.	Rate of retardation of epoch in circular measure per foot of descent.
feet.						
3 to 6	·1259	·1176	·1004	·1163	·09372	·06599
6 to 12	·1206	·1176	·1130	·1193	·06304	·06690
12 to 24	·1101	·1129	·1084	·1062	·06476	·06690
3 to 24	·1154	·1149	·1082	·1114	·06841	·06648

16. All the numbers here shown for each station would be equal, if the conditions of uniformity supposed in the theoretical solution were fulfilled. The discrepancies are, with the exception of one of the numbers for Craigleith Quarry, on the whole small; smaller, indeed, than might be expected when the very notable deviations of the true circumstances from the theoretical conditions are considered. The mean results over the 21 feet, shown in the last line, present very remarkable agreements,—the numbers derived from amplitudes being identical with that derived from epochs for the Calton Hill station, while the differences between the corresponding numbers for the two other stations are in each case only about three per cent. Taking that one number for the first station, and the mean of the slightly differing numbers derived from amplitudes and from epochs respectively for the second and third, we have undoubtedly very accurate determinations of the value of $\sqrt{\frac{\pi c}{k}}$ for the three stations, which are as follows :—

Calton Hill trap rock.	Experimental Garden sand.	Craigleith Quarry sandstone.
$\sqrt{\frac{\pi c}{k}}=\cdot1154$	$\sqrt{\frac{\pi c}{k}}=\cdot1098$	$\sqrt{\frac{\pi c}{k}}=\cdot06744$

A continuation of the observations at Calton Hill not only leads, as we shall see, to almost identical results, both by diminution of amplitude and by retardation, on the whole 21 feet,

but also reproduces some of the features of discrepance presented by the progress of the variation through the intermediate depths, and therefore confirms the general accuracy of the preceding results, for all the stations, so far as it might be questioned because of only five years' observations having been available. Further consideration of these results, and deduction of the conductivities of the different portions of the earth's crust involved, are deferred until after we have taken into account the further data for Calton Hill, to the reduction of which we now proceed.

[To be continued.]

V. *Meteorological Charts. By* Francis Galton, *Esq.**

[With a Plate.]

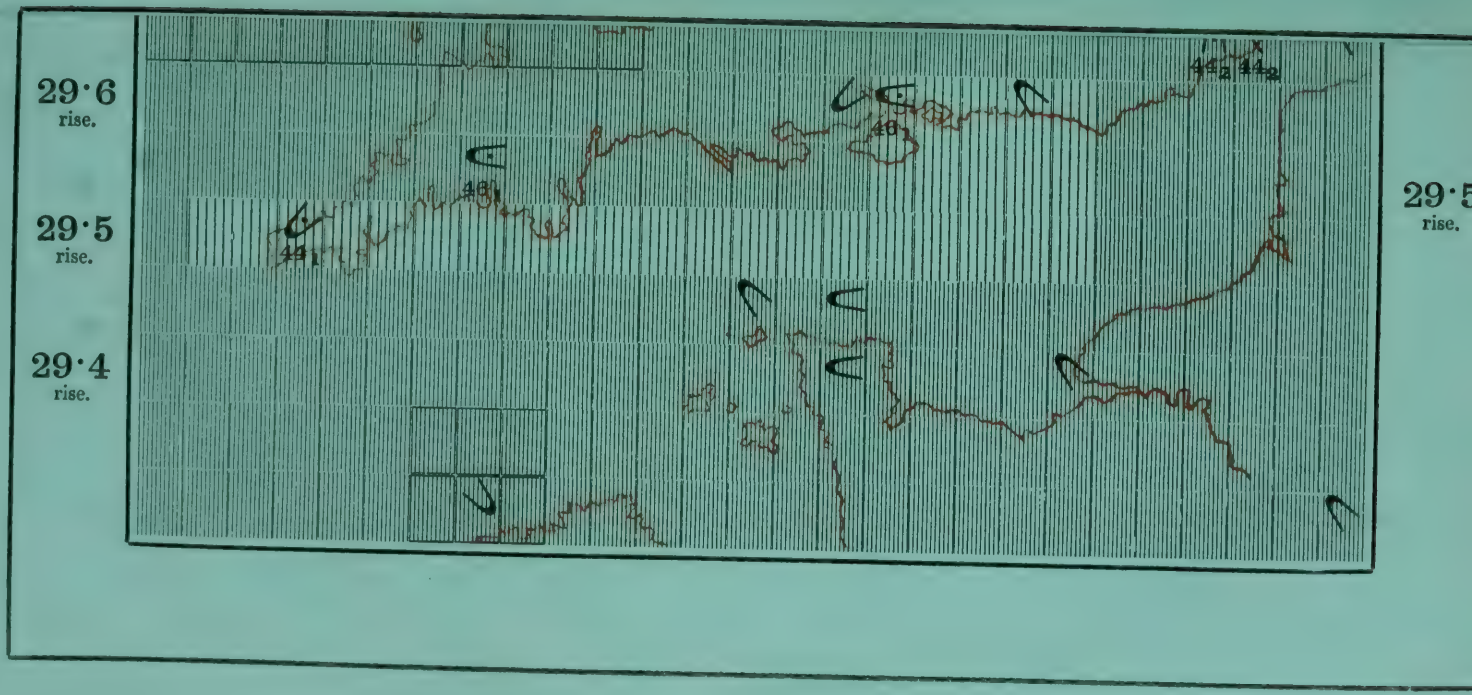
WHEN contemporary meteorological reports from numerous stations are printed one after another in a column (such as we may see in newspapers and certain foreign publications), they present no picture to the reader's mind. Lists of this description are therefore insufficient to do more than supply data which meteorological students must protract as they best can, upon a map, in some notation intelligible to themselves, at a considerable expense of labour and artistic skill.

It is needless to enlarge upon the serious obstacle which the necessity of doing this opposes to the pursuit of meteorology. It has sufficed to convert what might be a very popular science into a laborious and difficult study. We require means of printing, not lists of dry figures, but actual charts which should record meteorological observations pictorially and geographically, without sacrificing detail. It is then in the belief that an attempt I have just made to supply this desideratum might interest some of your readers, and perhaps lead to useful suggestions, that I forward the accompanying chart. (Plate II). It has been printed with moveable types, which I designed and caused to be cast; and I am much indebted to Mr. W. Spottiswoode, who printed it, for his aid in carrying out my ideas. The map simply incorporates the newspaper data of the day to which it refers, and was printed, not with any scientific object, but solely for the purpose of experiment.

Explanation of the Symbols.

The shade signifies cloud, of an amount proportional to its depth. The types with lines round them, , stand for rain. Cloud types have been interpolated where observations were

* Communicated by the Author.


29·6
rise.
29·5
rise.
29·4
rise.
29·5
rise.
44½
44½
46
46
44½

wanting. The horseshoes show the direction of the wind current: thus, ⊃ means wind *from* the west. An included spot ⊙, or line ⊃, or cross ⊕, respectively signify that the wind is gentle, moderate, or strong; where neither dot, line, nor cross are inserted, the force of the wind is unknown. Thermometrical data are expressed by figures, printed below the wind symbols. The first two figures of each set stand for the height of the ordinary thermometer, and the last figure (in a different type) for the difference between this and the thermometer with a wetted bulb. To save confusion of figures, barometer heights are not inserted on the face of the present map; but lines of equal barometric pressure have been deduced from the existing observations, and the places where lines corresponding to each integral one-tenth of an inch cut the marginal columns, have been marked. Thus a straight line joining the pair of figures, 29·7, is approximately the line of that pressure.

I do not consider the types here employed as forming a complete series. An additional shade for cloud is especially wanted.

It will be observed that no space would be lost by this mode of representation, supposing we possessed observations corresponding to every type space of the map.

42 Rutland Gate, S.W.

VI. *On the Curves situate on a Surface of the Second Order.* *By* A. Cayley, *Esq.**

A SURFACE of the second order has on it a double system of generating lines, real or imaginary; and any two generating lines of the first kind form with any two generating lines of the second kind a skew quadrangle. If the equations of the planes containing respectively the first and second, second and third, third and fourth, fourth and first sides of the quadrangle are $x=0$, $y=0$, $z=0$, $w=0$, and if the constant multipliers which are implicitly contained in x, y, z, w respectively are suitably determined, then the equation of the surface of the second order (or say for shortness the quadric surface) is $xw-yz=0$.

Assume $\frac{y}{x}=\frac{\mu}{\lambda}$, $\frac{z}{x}=\frac{\nu}{\rho}$, then $\frac{\mu}{\lambda}, \frac{\nu}{\rho}$, or say $(\lambda, \mu, \nu, \rho)$, may be regarded as the coordinates of a point on the quadric surface; we in fact have $x:y:z:w=1:\frac{\mu}{\lambda}:\frac{\nu}{\rho}:\frac{\mu\nu}{\lambda\rho}$, or what is the same

* Communicated by the Author.

thing, $=\lambda\rho : \mu\rho : \nu\lambda : \mu\nu$. The four quantities $(\lambda, \mu, \nu, \rho)$ are for symmetry of notation used as coordinates; but it is to be throughout borne in mind that the absolute magnitudes of λ and μ, and of ν and ρ are essentially indeterminate; it is only the ratios $\lambda : \mu$ and $\nu : \rho$ that we are concerned with.

An equation of the form

$$(*\chi\lambda, \mu)^p(\nu, \rho)^q = 0,$$

that is, an equation homogeneous of the degree p as regards (λ, μ), and homogeneous of the degree q as regards (ν, ρ), represents a curve on the quadric surface; and this curve is of the order $p+q$. In fact, combining with the equation of the curve the equation of an arbitrary plane

$$Ax + By + Cz + Dw = 0,$$

this equation, expressed in terms of the coordinates $(\lambda, \mu, \nu, \rho)$, is

$$A\lambda\rho + B\mu\rho + C\nu\lambda + D\mu\nu = 0;$$

or, as it is more conveniently written,

$$\begin{vmatrix} C, & D\chi\lambda, \mu)(\nu, \rho) = 0; \\ A, & B \end{vmatrix}$$

and if from this and the equation of the curve we eliminate $\lambda : \mu$ or $\nu : \rho$, say the second of these quantities, we obtain

$$(*\chi\lambda, \mu)^p(-A\lambda - B\mu, C\lambda + D\mu)^q = 0,$$

which is of the order $p+q$ in (λ, μ); and $\lambda : \mu$ being known, $\nu : \rho$ is linearly determined. There are thus $p+q$ systems of values of the coordinates, or the plane meets the curve in $p+q$ points; that is, the curve is of the order $p+q$.

A linear equation $A\lambda + B\mu = 0$ gives a generating line, say of the first kind, of the quadric surface, and a linear equation $C\nu + D\rho = 0$ gives a generating line of the second kind. And by combining the one or the other of these equations with the equation of the curve, it is at once seen that the curve meets each generating line of the first kind in q points, and each generating line of the second kind in p points.

Consider the curves of the order n: the different solutions of the equation $p+q=n$ give different species of curves. But the solution $(n, 0)$ gives only a system of n generating lines of the first kind, and the solution $(0, n)$ gives only a system of generating lines of the second kind. And in general the solutions (p, q) and (q, p) give species of curves which are related, the one of them to the generating lines of the first and second kinds, in the same way as the other of them to the generating

lines of the second and first kinds; and they may be considered as correlative members of the same species. The number of distinct species is thus $\frac{n-1}{2}$ or $\frac{n}{2}$, according as n is odd or even; for $n=3$ we have the single species (2, 1) or (1, 2); for $n=4$, the two species (1, 3) or (3, 1), and (2, 2); for $n=5$, the two species (4, 1) or (1, 4), and (3, 2) or (2, 3); and so on. Thus for $n=3$, the species (2, 1) is represented by an equation of the form

$$(a, b, c\mathcal{X}\lambda, \mu)^2\nu+(a', b', c'\mathcal{X}\lambda, \mu)^2\rho=0,$$

which belongs to a cubic curve in space. To show *à posteriori* that this is so, I observe that the equation expressed in terms of the original coordinates (x, y, z, w) is

$$x(a, b, c\mathcal{X}x, y)^2+z(a', b', c'\mathcal{X}x, y)^2=0,$$

which by means of the equation $xw-yz=0$ of the quadric surface is reduced to

$$(a, b, c\mathcal{X}x, y)^2+a'xz+2b'yz+c'yw=0;$$

and this is the equation of a quadric surface intersecting the quadric surface $xw-yz=0$ in the line $x=0$, $y=0$; and therefore also intersecting it in a cubic curve.

For $n=4$, I take first the species (2, 2) which is represented by an equation of the form

$$(a, b, c\mathcal{X}\lambda, \mu)^2\nu^2+2(a', b' c'\mathcal{X}\lambda, \mu)^2\nu\rho+(a'', b'', c''\mathcal{X}\lambda, \mu)^2\rho^2=0,$$

which in fact belongs to a quartic curve, the intersection of two quadric surfaces. For, reverting to the original coordinates, the equation becomes

$$(a, b, c\mathcal{X}x, y)^2x^2+2(a', b', c'\mathcal{X}x, y)^2xz+(a'', b'', c''\mathcal{X}x, y)^2z^2=0,$$

which by means of the equation $xw-yz=0$ of the quadric surface is at once reduced to

$$(a, b, c\mathcal{X}x, y)^2+2a'xz+4b'yz+2c'yw+a''z^2+2b''zw+c''w^2=0,$$

which is the equation of a quadric surface intersecting the given quadric surface $xw-yz=0$ in the curve in question.

Consider next the species (3, 1) represented by an equation of the form

$$(a, b, c, d\mathcal{X}\lambda, \mu)^3\nu+(a', b', c', d'\mathcal{X}\lambda, \mu)^3\rho=0,$$

which is the other species of quartic curve situate on only a single quadric surface. Reverting to the original coordinates, the

equation becomes

$$(a, b, c, d)(x, y)^3 x + (a', b', c', d')(x, y)^3 z = 0.$$

And by means of the equation $xw - yz = 0$ of the quadric surface this is reduced to

$$(a, b, c, d)(x, y)^3 + d' x^2 z + 3b' xyz + 3c' y^2 z + d' y^2 w = 0,$$

which is the equation of a cubic surface containing the line $(x=0, y=0)$ twice, and therefore along this line touching the quadric surface $xw - yz = 0$; and consequently intersecting it besides in a quartic curve. And in like manner for the curves of the fifth and higher orders which lie upon a quadric surface.

The combination of the equations

$$(*)(\lambda, \mu)^p (\nu, \rho)^q = 0,$$
$$(*')(\lambda, \mu)^{p'} (\nu, \rho)^{q'} = 0,$$

shows at once that two curves on the same quadric surface of the species (p, q) and (p', q') respectively intersect in a number $(pq' + p'q)$ of points. Thus if the curves are (1, 0) and (1, 0), or (0, 1) and (0, 1), *i. e.* generating lines of the same kind, the number of intersections is $1 \cdot 0 + 0 \cdot 1 = 0$; but if the curves are (1, 0) and (0, 1), *i. e.* generating lines of different kinds, the number of intersections is $1 \cdot 1 + 0 \cdot 0 = 1$.

The notion of the employment of hyperboloidal coordinates presented itself several years ago to Prof. Plücker (see his paper "Die analytische Geometrie der Curven auf den Flächen zweiter Ordnung und Classe," Crelle, vol. xxxiv. pp. 341–359 [1847]); but the systems made use of, *e. g.* $\xi = -\frac{d}{\mu}\frac{z}{y}$, $\eta = -\frac{d}{\mu}\frac{z}{x}$, with $z(z+d) + \mu xy = 0$ for the equation of the surface of the second order, is less simple; and the question of the classification of the curves on the surface is not entered on.

2 Stone Buildings, W.C.,
May 24, 1861.

VII. *On some Experiments connected with* Dove's *Theory of Lustre.* *By* Prof. O. N. Rood, *of Troy**.

IN the *Farbenlehre*, p. 177, Prof. Dove writes, "In every case where a surface appears lustrous, there is always a transparent or translucent reflecting stratum of minor intensity, through which we see another body. It is therefore externally

* From Silliman's American Journal for May 1861.

reflected light in combination with internally reflected or dispersed light, whose combined action produces the idea of lustre."

Thus by combining in the stereoscope two projections of a pyramid, one drawn in black lines on a white ground, the other in white lines on black ground, Dove found that the pyramid appeared lustrous as though made of graphite. [To me it recalls rather the idea of highly polished glass.] He found also that a yellow and blue surface, when combined in the stereoscope and viewed through a plate of violet glass, produced, in the act of combination, the idea of a polished metal.

Similar to Dove's theory of lustre is that of Prof. Reute*.

This view of the nature of lustre opens to us the possibility of reproducing by the stereoscopic combination of suitably coloured surfaces, the individual lustre and appearance of gold, copper, brass, &c.; it also affords us a means of examining separately the components which *may produce* the appearances peculiar to each.

1. I combined in the stereoscope on white or on black grounds, a piece of tinfoil one inch square with a piece of yellow paper of the same size. The value of the tint on the chromatic circles of Chevreul was, 1st circle, orange-yellow, No. 4. When the field containing the tinfoil was somewhat shaded by the hand or otherwise, the surface seen in the stereoscope could not be distinguished from gold-leaf. The union of the images took place as readily and the illusion was as strong with persons unaccustomed to the use of the instrument.

2. By combining in the same way tinfoil with orange-tinted paper (1st circle, orange), the lustre and appearance of copper is imitated.

3. Tinfoil in the act of combination with Nos. 14 and 15 of the red and black scale imitate bismuth.

4. Tinfoil or silver-foil in the act of combination with ultramarine paper appears scarcely blue, rather black like foliated graphite.

5. Gold-leaf in combination with paper of a tint nearly that of the green of the 1st circle imitated murexide.

6. Gold-leaf in combination with ultramarine paper resembled a surface of graphite.

Upon substituting dark grey paper for the tinfoil the same effects in degree were not produced, owing, as it seemed to me, to the fact that the well-known texture and appearance of the paper forcing itself on the attention, precluded the idea of anything metallic. To remove this difficulty I employed two means:—

* *Das Stereoscop*; C. G. Th. Reute, Leipzig, 1860.

1. A crumpled sheet of tinfoil was photographed, and from the negative, prints were taken by the "ammonia-nitrate process," which were toned to the so-called black of the photographers. This furnished dark paper upon whose surface was an accurate drawing of the irregularities characteristic of metallic foil; the surface of the paper was of course wholly without lustre.

(*a*) Upon combining, in black or white fields, a square inch of one of those photographs with the above-mentioned yellow paper, and shading the photograph a little, a representation of gold was obtained but little inferior to that given by the use of the real tinfoil.

(*b*) This photographic paper in combination with orange paper (1st circle, orange) made an imitation of metallic copper.

(*c*) The ultramarine paper in combination with the photograph of tinfoil gave a striking imitation of foliated graphite. The blue colour is perceived much less than would be expected.

2. The surface of a plate of brass 1 inch square was polished, and then rather heavily scratched by a coarse file. Into the scratches a small amount of yellow or white oil paint was rubbed, and upon this prepared surface dark grey or black paper was laid, and the whole submitted to the action of a press as in copper-plate printing. By this means a drawing of a scratched metallic surface was transferred to paper. These markings serve also to enable the observer much more easily to direct his attention simultaneously to the two impressions presented.

(*a*) Upon combining dark grey paper (black and white scale, Nos. 18, 19, 20) prepared in this way with the above-mentioned yellow paper, the appearance of a polished, scratched plate of gold was obtained.

(*b*) When these dark prepared papers were combined with yellow paper coloured by gamboge (yellow and black scale, No. 9), the appearance and lustre of brass were obtained.

According to Dove's theory the darker surface in the stereoscope represents the dispersed light, the brighter, that regularly reflected. As the polish of a metallic surface is proportional to the smallness in amount of the light it disperses, we should be led to expect that by varying the shade of the black paper, we should be able to alter the apparent degree of polish of these imitated metallic surfaces.

This is the case: yellow paper (1st circle, orange-yellow No. 4), in combination with black (No. 21), gives the idea of a very highly polished golden surface; as we descend in the scale, the lustre and resemblance to polished metal regularly diminishes till at grey, No. 8, almost no effect like gold is to be perceived.

On the other hand, by diminishing the brightness of the yellow paper, the black tint remaining constant, the idea of a polished golden plate in the shade, or so placed as to reflect the image of some dark object, is produced. Thus we may descend through the circles of Chevreul to the 7th, when by combining the orange-yellow of that circle with No. 21 of the grey scale, the idea of a golden plate much shaded is produced. I constructed tables expressing the effects produced by varying the intensity of the two components; but it is not worth while to introduce them here.

As we are accustomed to see gold tinted variously from nearly a yellow as in gold-leaf, to almost a copper hue as in some specimens of our American coin, so the tint of the paper placed in the stereoscope may be varied within certain limits, without greatly affecting the results.

Prof. Helmholtz, in his admirable work on Physiological Optics*, mentions that by a peculiar arrangement he was able to cause the homogeneous golden-yellow light of the spectrum to appear brown, proving thus that the tint brown is only weak yellow light. These stereoscopic experiments give us, on the other hand, the means of apparently converting brown into a metallic golden yellow; for many specimens of even brown wrapping paper, when combined in the stereoscope with very black prepared paper, acquire the lustre and appearance of yellow plates in the shade, and reflecting images of dark objects.

In the same manner, and corresponding to the investigations of Helmholtz, I found that the stereoscopic union of black glazed paper with red (No. 14, red and black scale), imitated with surprising perfection the appearance of a glazed plate of chocolate.

The chromatic scales of Chevreul furnish us with a ready means of combining in rapid succession in the stereoscope a great number of definite tints; thus by cutting in a card-board two parallel apertures $\frac{3}{10}$ inch broad and 1 inch long, their distance apart being 2·6 inches, and pasting under one of them black prepared paper, the other can be brought over any desired tint and the effect noted.

1. In this way I found that a pretty good representation of the appearance of slightly tarnished lead was produced by the stereoscopic union of grey No. 18 and No. 4 on the blue-violet and black scale.

2. A somewhat inferior imitation of antimony was given by No. 1 blue and black scale, with grey Nos. 18 to 20, or by using No. 17 blue and black scale with white.

* P. 281. *Physiologische Optik* (*Encyklopädie der Physik.* Leipzig, 1860).

3. Tarnished zinc surfaces may be imitated by the use of grey No. 5 with No. 18 blue and black scale.

4. Ultramarine paper, with some of the lighter violet-blues, gave an imitation of blue glass. The idea of blue polished glass was also obtained by using in combination with the ultramarine paper No. 1 of the yellow and black scale.

I will mention here that the stereoscopic union of this blue with yellow paper, never induced in my mind the idea of green.

I made some experiments to ascertain how far the *stereoscopic* mixture of two masses of different coloured light corresponded to their true mixture by the method of rapid rotation, use being made of the imitations above described. It is however so difficult to compare a varying with a fixed tint that I will not record the results obtained; in many cases a certain moderate amount of agreement in the resultant tints was observed. Brücke found, when a deeply-coloured yellow glass was held before one eye and a blue cobalt glass before the other, that a landscape viewed through this combination was simply darkened in appearance. I repeated this experiment with similar glasses, and obtained a like result; objects appeared darkened, but in their natural colours, though sometimes the blue or yellow tint predominated a little. But when I presented to a *single* eye these two masses of light, a very different result was obtained; the plates of glass were attached to a blackened disk opposite suitable perforations, and it was set in rapid rotation; a landscape viewed through it appeared deep purple, though not a trace of this colour was to be perceived in the binocular use of these glasses.

When these two glasses were held before the same eye, a landscape viewed through them was very much darkened but scarcely coloured.

Sir David Brewster's Theory of Lustre.

Sir David Brewster opposes Dove's theory of lustre, as he has found that when black and white surfaces without drawings are combined in the stereoscope, no lustre is produced. The lustre, then, according to this philosopher, is due not to one mass of light passing through another, but to the effort of the eyes to combine the stereoscopic pictures.

Admitting the correctness of Sir David's experiment, Dove has shown that the objection founded on it is without weight (p. 3, Optical Studies).

In repeating Brewster's experiment I always obtain the *opposite result*; in combining uniform black and white surfaces, without drawings, I always obtain a distinct impression of lustre, like that of the blackened mirror of a polariscope, and in strict accordance with Dove's theory; when the black field is so dark-

ened that *no light* is sent from it to the eye, this lustre vanishes, and the white paper alone is perceived. This disagreement is not a cause of astonishment when we reflect that De Haldat's original experiment waited nearly half a century for confirmation.

To Brewster's own theory, the simple objection, which has already been made by others, that we daily perceive lustre plainly with one eye, would seem sufficient.

Production of Lustre in Monocular Vision.

I proceed now to describe some experiments where by the action upon a single eye of two masses of light of unequal intensity, the idea of lustre is produced.

1. If a disk of coloured card-board, out of which a number of sectors has been removed, be made to rotate rapidly, and an object be viewed through it by a single eye, two masses of light will reach the eye, which apparently proceed from the object; one is reflected from the surface of the disk, the other emanates from the object behind the disk, and passes through the first mass of light. Dark objects viewed in this way assume to me, to a small extent, an appearance like that of blackened glass. The effect is not at all striking, and would be overlooked by many persons; I therefore prepared paper in a peculiar way, so as to imitate distantly the appearance of foliated graphite or crumpled mica.

White smooth drawing-paper was rubbed over irregularly with a brush *slightly* moistened with a weak wash of India ink or lampblack; when dry, another wash of a deeper hue applied as before, care being taken to leave many small spots untouched. The final wash was laid on with pure black. If the brush be kept nearly dry and passed only lightly over the paper, it is easy to obtain a surface bearing some very distant resemblance to the minerals above mentioned; it is of course without lustre. Similar papers were prepared with red and blue water colours.

When these papers were held behind disks of ultramarine or orange-tinted paper, from which equal alternate sectors had been removed, and which were revolving at such rates that their surfaces seemed uniform, or at lower rates, they often appeared, to a single eye, highly lustrous. This was true of the prepared paper in a *state of rest*; when moved slightly by the hand it glittered strongly. Dark photographs of tinfoil held behind a revolving disk of ultramarine paper and viewed by a single eye, assume often to a striking degree the lustre and appearance of foliated graphite.

2. If a piece of this peculiarly blackened paper $\frac{1}{2}$ an inch square be placed in a blue field (rather light ultramarine paper)

and be steadily regarded for some minutes by one eye, it assumes a red-orange hue, and appears suspended over the blue paper and nearer to the eye than the latter; at the same instant it appears lustrous like crumpled mica. The illusion with me often lasts half a minute in great perfection; this is particularly the case when the eye is not quite accurately focused on the paper.

3. If a sheet of this prepared paper be brightly illuminated by light from a window, and be held so near one eye as to produce indistinct vision, it often apparently becomes highly lustrous. In this case enlarged images of the white and grey points are formed on the retina, which overlap, so that again we have two masses of light, one passing through the other.

4. If a *roll* of black paper like the above, but coarser in its markings, be brightly illuminated on one side and viewed through deeply coloured plates of glass (red, green, blue), in a few seconds it appears lustrous, resembling a roll of polished zinc which has been irregularly and deeply corroded by an acid. Upon removing the glass, the surface of the paper appears lustrous for an instant.

5. A sheet of the finer variety of this prepared paper viewed through a large rhomb of calc-spar, gives often in spots the appearance of lustre, particularly when the head of the observer, or the rhomb, is slightly moved. Some persons compared this to the appearance of water.

It would seem probable that in all cases where two masses of light reach a single eye, one passing through the other, particularly when there is any *perception of their individuality*, that the appearance of more or less lustre is produced, though from habit we often overlook it. Thus Helmholtz remarks* (upon the combination of two coloured surfaces in monocular vision by means of a simple instrument he figures), "It is particularly favourable when the drawings, or spots on the two surfaces, are made to shift their position. Then we often believe that we see both colours simultaneously in the same place, the one through the other. We have an impression in such cases of seeing objects through a coloured veil or reflected from a coloured surface."

I found, in fact, that by placing stereographs consisting of coloured paper for one eye and a photographic drawing of tinfoil for the other in this instrument, that lustre could be perceived, particularly with the imitations of copper.

The diagram represents the instrument referred to; it consists of a plate of glass, P, with parallel sides, which is properly supported over a blackened board B. Differently coloured papers are placed at K and Y; one is seen through the plate, and the

* *Physiologische Optik*, p. 273.

other by reflexion from it. The images are made to overlap, and their intensity is regulated by altering their distance from F.

Analogous to this is the observation of Brewster*. Speaking of uniting similar pictures (patterns on hanging-paper) in binocular vision, he remarks, "The surface of it (the wall) seems slightly curved. It has a *silvery transparent aspect*." Here the images (though of the same intensity, &c.) moving with each slight movement of the head induces in the mind the idea of one object seen through another.

In closing, I will remark that while many of the experiments above mentioned are easily repeated, others require considerable practice in this kind of observation.

VIII. *Concluding Paper on Tactic. By* J. J. Sylvester, *M.A., F.R.S., Professor of Mathematics at the Royal Military Academy, Woolwich*†.

IN my tactical paper in the May Number of the Magazine, I considered the number of groupings and of types of groupings of *synthemes* formed out of triads of three *nomes* of three elements each. The first example of considering the *ensemble* of the groupings of a defined species of synthemes (each of such groupings being subjected to satisfy a certain exhaustive condition) was, as already stated, furnished by me in this Magazine, April 1844. In that case the synthemes consisted of duads belonging to a single nome of 6 elements, and the total number of the groupings was observed to be 6, all contained in one type or family. The total number of synthemes in that instance being 15, and there being 6 groupings of 5 synthemes each, it followed that in the whole family every syntheme is met with twice over; once in one grouping, and once in another. In the case treated of in my last communication to the Magazine, the total number of the synthemes of the kind under consideration is 36 (for it may easily be shown that the number of synthemes of n-nomial n-ads of n nomes of q elements each is $(1.2.3 \ldots q)^{n-1}$); and as each grouping contains 9 synthemes, these 36 are distributed *without repetition* between the 4 groupings of the smaller of the two natural species,—a phenomenon of a kind here met with for the first time in the study of *syntax*. If now we go on (as a natural and irrepressible curiosity urges) to ascertain the groupings of the synthemes of *binomial* triads of the same 3

* The Stereoscope, p. 91. London, 1856.

† Communicated by the Author.

nomes of three elements each, we advance just one step further in the direction of type-complexity; that is to say, we meet with the existence of 3, and not more than 3, types or species in which all such groupings are comprised. The investigation by which this is made out appears to me well worthy to be given to the world, as affording an example of a new and beautiful kind of analysis proper to the study of *tactic*, and thus lighting the way to the further opening up of this fundamental doctrine of mathematic, the science of necessary relations, of which, combined with logic (if indeed the two be not identical), tactic appears to me to constitute the main stem from which all others, including even arithmetic itself, are derived and secondary branches. The key to success in dealing with the problems of this incipient science (as I suppose of most others) must be sought for in the construction of an apt and expressive notation, and in the discovery of language by force of which the mind may be enabled to lay hold of complex operations and mould them into simple and easily transmissible forms of thought. I must then entreat the indulgence of the reader if, in this early grappling with the difficulties of a new language and a new notation, I may occasionally appear wanting in absolute clearness and fullness of expression.

Let us, as before, represent the nine elements by the numbers from 1 to 9, and suppose the *nomes* to be 1, 2, 3 : 4, 5, 6 : 7, 8, 9.

If we take any syntheme formed out of the *binomial* triads belonging to the above nomes, and if out of such syntheme we omit the elements 1, 2, 3 (belonging to the 1st nome) wherever they occur, the slightest consideration will serve to show that the synthemes thus denuded will assume the *form* $l.m.r, p.q, n$, where l, m, r may be regarded as belonging to one of the remaining nomes, and p, q, n to the other. The total number of synthemes in a grouping which contains all the binomial triads is 18, because the total number of these triads is 54; and consequently it will be seen that every grouping will in fact consist of the same *framework*, so to say, of combinations of elements belonging to the second and third nomes variously compounded with the elements of the first nome.

This framework may be advantageously divided into two parts, each containing nine terms, and which I shall call respectively U and $\dot{\text{U}}$. Thus by U I shall understand the nine arrangements following:—

4.5.7, 8.9, 6; 4.5.8, 7.9, 6; 4.5.9, 7.8, 6
5.6.7, 8.9, 4; 5.6.8, 7.9, 4; 5.6.9, 7.8, 4
6.4.7, 8.9, 5; 6.4.8, 7.9, 5; 6.4.9, 7.8, 5

each *imperfect* or defective syntheme being separated from the next by a semicolon, or else by a change of line. So by $\dot{\text{U}}$

I shall understand the *complementary* part of the framework, viz. :—

8.9.6, 4.5, 7; 7.9.6, 4.5, 8; 7.8.6, 4.5, 9
8.9.4, 5.6, 7; 7.9.4, 5.6, 8; 7.8.4, 5.6, 9
8.9.5, 6.4, 7; 7.9.5, 6.4, 8; 7.8.5, 6.4, 9

It is of cardinal importance to notice that the order in which the *imperfect synthemes* are arranged in U and $\dot{U}$ is one of absolute reciprocity. It is in this reciprocity, and in the fact of U or $\dot{U}$ being each in *strict regimen* (so to say) with the other, that the cause of the success of the method about to be applied essentially resides.

The slightest reflection will serve to show that every *complete* syntheme of the kind required will be of the form

$$\left|\begin{array}{c} U \times P \\ \dot{U} \times \dot{P} \end{array}\right|$$

where the symbolical multipliers P and $\dot{P}$ are each of them some one of the forms (by no means necessarily the *same*) represented generally by the framework of defective synthemes hereunder written (defective in the sense that all the elements of the second and third nomes are supposed to be omitted),

, *a*, *bc*; , *b*, *ca*; , *c*, *ab*
, *b*, *ca*; , *c*, *ab*; , *a*, *bc*
, *c*, *ab*; , *a*, *bc*; , *b*, *ca*

or else by the cognate framework

, *a*, *bc*; , *c*, *ab*; , *b*, *ca*
, *b*, *ca*; , *a*, *bc*; *c*, *ab*
, *c*, *ab*; , *b*, *ca*; *a*, *bc*

where *a*, *b*, *c* are identical in some order or another with the elements of the first nome, viz. 1, 2, 3; so that there are six different systems of *a*, *b*, *c* in each of these two frameworks.

No other combination of the elements in U or $\dot{U}$ (all of which belong to the second and third nomes) with the elements in the first nome is possible; for any such combination would involve the fact of a *repetition* of the same *triad* or triads in the same grouping, contrary to the nature of a grouping. Hence, then, the number of forms of P and of $\dot{P}$ being twice six, or 12, we at once perceive that the total number of groupings is 12×12, or 144.

But now comes the more difficult question of ascertaining between how many distinct species or types these groupings are distributed. If we study the form of P or $\dot{P}$, it is obvious that

it will be completely and distinctively *denoted in brief* by the twelve forms arising from the development of

$$\begin{matrix} a & b & c \\ b & c & a \\ c & a & b \end{matrix} \quad \text{and} \quad \begin{matrix} a & c & b \\ b & a & c \\ c & b & a \end{matrix} \quad ; \text{ videlicet}$$

(1)	(2)	(3)	(4)	(5)	(6)
1 2 3	2 3 1	3 1 2	2 1 3	1 3 2	3 2 1
2 3 1	3 1 2	1 2 3	1 3 2	3 2 1	2 1 3
3 1 2	1 2 3	2 3 1	3 2 1	2 1 3	1 3 2

(7)	(8)	(9)	(10)	(11)	(12)
1 3 2	2 1 3	3 2 1	2 3 1	1 2 3	3 1 2
2 1 3	3 2 1	1 3 2	1 2 3	3 1 2	2 3 1
3 2 1	1 3 2	2 1 3	3 1 2	2 3 1	1 2 3

which we may for facility of future reference denote by

$$\pi_1\, \pi_2\, \pi_3\, \pi_4\, \pi_5 \ldots \pi_{12}.$$

Now as regards the types: since the order of the elements in one nome is entirely independent of the order of the elements in any other, it is obvious that it is not the particular form of P or of $\dot{\mathrm{P}}$ which can have any influence on the form of the type, but solely the relation of P and $\dot{\mathrm{P}}$ to one another. In order then to fix the ideas, I shall for the moment consider P equal to

$$\begin{matrix} 1 & 2 & 3 \\ 2 & 3 & 1 \\ 3 & 1 & 2 \end{matrix}$$

This at once enables us to fix *a limit* to the number of distinct types. In the first place, the essentially distinct FORMS of the first column in $\dot{\mathrm{P}}$, with respect to that of P, may be sufficiently represented by taking the two columns identical, or differing by a single interchange, or else having no two elements in the same place. Hence $\dot{\mathrm{P}}$, so far as the ascertainment of types is concerned, may be limited to the six forms following:—

(α)	(γ)	(ϵ)
1 2 3	2 1 3	2 3 1
2 3 1	1 3 2	3 1 2
3 1 2	3 2 1	1 2 3

(β)	(δ)	(η)
1 3 2	2 3 1	2 1 3
2 1 3	1 2 3	3 2 1
3 2 1	3 1 2	1 3 2

But again, since (β) and (η) are each derivable from (α) (the assumed form of P) by an interchange of two columns *inter se*,

it is clear that, as regards distinction of type, $\eta=\beta$, and consequently there are only *at utmost* five types remaining, which may be respectively described by the symbols

$$\left|\begin{matrix}U\alpha\\ \dot{U}\alpha\end{matrix}\right| \quad \left|\begin{matrix}U\alpha\\ \dot{U}\beta\end{matrix}\right| \quad \left|\begin{matrix}U\alpha\\ \dot{U}\gamma\end{matrix}\right| \quad \left|\begin{matrix}U\alpha\\ \dot{U}\delta\end{matrix}\right| \quad \left|\begin{matrix}U\alpha\\ \dot{U}\epsilon\end{matrix}\right|$$

It must be noticed that α comprehends or typifies the squares numbered 1; β those numbered 7, 8, 9; γ those numbered 4, 5, 6; δ those numbered 10, 11, 12; ϵ those numbered 2, 3.

I say designedly that the number of types is *at utmost* limited to these five. But it by no means follows that the number is so great as five; for it will not fail to be borne in mind that these differences have reference to the peculiar mode in which we have chosen to decompose in idea each syntheme, by viewing it as a symbolical product of an arrangement containing only the elements of the second and third nomes by an arrangement containing only those of the first nomes. But the nomes are interchangeable, and therefore it may very well be the case that two types which appear to be distinct are in reality identical, their elements in the groupings appertaining to such types having absolutely analogous relations to different orderings of the nomes, so that the groupings will be convertible into each other by permutations among the given elements. We must therefore ascertain how the above types, or any specific forms of them, come to be represented when we interchange the first nome with either of the other two, or, to fix the ideas, let us say with the second.

To effect this, let $U\alpha$, $\dot{U}\alpha$, $\dot{U}\beta$, $\dot{U}\gamma$, $\dot{U}\delta$, $\dot{U}\epsilon$ be actually expanded; by the performance of the symbolical multiplications we obtain—

$$U\alpha = \left|\begin{matrix}4.5.7\ 8.9.1\ 6.2.3; & 4.5.8\ 7.9.2\ 6.1.3; & 4.5.9\ 7.8.3\ 6.2.1\\ 5.6.7\ 8.9.2\ 4.1.3; & 5.6.8\ 7.9.3\ 4.1.2; & 5.6.9\ 7.8.1\ 4.2.3\\ 6.4.7\ 8.9.3\ 5.1.2; & 6.4.8\ 7.9.1\ 5.2.3; & 6.4.9\ 7.8.2\ 5.1.3\end{matrix}\right|$$

$$\dot{U}\alpha = \left|\begin{matrix}8.9.6\ 4.5.1\ 7.2.3 & 7.9.6\ 4.5.2\ 8.1.3 & 7.8.6\ 4.5.3\ 9.2.1\\ 8.9.4\ 5.6.2\ 7.1.3 & 7.9.4\ 5.6.3\ 8.1.2 & 7.8.4\ 5.6.1\ 9.2.3\\ 8.9.5\ 6.4.3\ 7.2.1 & 7.9.5\ 6.4.1\ 8.2.3 & 7.8.5\ 6.4.2\ 9.1.3\end{matrix}\right|$$

$$U\beta = \left|\begin{matrix}8.9.6\ 4.5.1\ 7.2.3 & 7.9.6\ 4.5.3\ 8.1.2 & 7.8.6\ 4.5.2\ 9.1.3\\ 8.9.4\ 5.6.2\ 7.1.3 & 7.9.4\ 5.6.1\ 8.2.3 & 7.8.4\ 5.6.3\ 9.2.1\\ 8.9.5\ 6.4.3\ 7.2.1 & 7.9.5\ 6.4.2\ 8.1.3 & 7.8.5\ 6.4.1\ 9.2.3\end{matrix}\right|$$

$$\dot{U}\gamma = \left|\begin{matrix}8.9.6\ 4.5.2\ 7.1.3 & 7.9.6\ 4.5.1\ 8.2.3 & 7.8.6\ 4.5.3\ 9.1.2\\ 8.9.4\ 5.6.1\ 7.2.3 & 7.9.4\ 5.6.3\ 8.1.2 & 7.8.4\ 5.6.2\ 9.1.3\\ 8.9.5\ 6.4.3\ 7.1.2 & 7.9.5\ 6.4.2\ 8.1.3 & 7.8.5\ 6.4.1\ 9.2.3\end{matrix}\right|$$

$$\dot{U}\delta = \left|\begin{matrix}8.9.6\ 4.5.2\ 7.1.3 & 7.9.6\ 4.5.3\ 8.1.2 & 7.8.6\ 4.5.1\ 9.2.3\\ 8.9.4\ 5.6.1\ 7.2.3 & 7.9.4\ 5.6.2\ 8.1.3 & 7.8.4\ 5.6.3\ 9.1.2\\ 8.9.5\ 6.4.3\ 7.1.2 & 7.9.5\ 6.4.1\ 8.2.3 & 7.8.5\ 6.4.2\ 9.1.3\end{matrix}\right|$$

$$\dot{U}\epsilon = \left|\begin{matrix}8.9.6\ 4.5.2\ 7.1.3 & 7.9.6\ 4.5.3\ 8.1.2 & 7.8.6\ 4.5.1\ 9.2.3\\ 8.9.4\ 5.6.3\ 7.1.2 & 7.9.4\ 5.6.1\ 8.2.3 & 7.8.4\ 5.6.2\ 9.1.3\\ 8.9.5\ 6.4.1\ 7.2.3 & 7.9.5\ 6.4.2\ 8.1.3 & 7.8.5\ 6.4.3\ 9.1.2\end{matrix}\right|$$

Let us form a *framework* with the nomes 1.2.3, 7.8.9 exactly similar to that which we formed before with 4.5.6, 7.8.9, and let V, $\dot{V}$ be its two parts respectively analogous to U, $\dot{U}$, we thus obtain for $\dot{V}$,

1.2.7, 8.9, 3;	7.2.8, 7.9, 3;	1.2.9, 7.8, 3
2.3.7, 8.9, 1;	2.3.8, 7.9, 1;	2.3.9, 7.8, 1
3.1.7, 8.9, 2;	3.1.8, 7.9, 2;	3.1.9, 7.8, 2

and for V,

8.9.3, 1.2, 7;	7.9.3, 1.2, 8;	7.8.3, 1.2, 9
8.9.1, 2.3, 7;	7.9.1, 2.3, 8;	7.8.1, 2.3, 9
8.9.2, 3.1, 7;	7.9.2, 3.1, 8;	7.8.2, 3.1, 9

We must now perform the unwonted process of symbolical division, and obtain the quotients of $U\alpha$ by V, and of $\dot{U}\alpha$, $\dot{U}\beta$, $\dot{U}\gamma$, $\dot{U}\delta$, $\dot{U}\epsilon$ by $\dot{V}$ (it will of course be perceived that it is known *à priori* that the dividend forms of arrangement are actual multipliers of the divisors V and $\dot{V}$). In writing down the results of these divisions, which will consist exclusively of elements belonging to the nome 4.5.6, and of which each term will be of the form $d, e.f$, we may, analogously to what we have done before for greater brevity, write down only the single element (d), and omit the residue (ef), which is determined when (d) is determined. We shall thus obtain the quotients following:—

$$\frac{U\alpha}{V} = \begin{matrix} 5\ 4\ 6 \\ 6\ 5\ 4 \\ 4\ 6\ 5 \end{matrix}$$

$$\frac{\dot{U}\alpha}{\dot{V}} = \begin{matrix} 5\ 4\ 6 \\ 6\ 5\ 4 \\ 4\ 6\ 5 \end{matrix} \qquad \frac{\dot{U}\beta}{\dot{V}} = \begin{matrix} 5\ 6\ 4 \\ 6\ 4\ 5 \\ 4\ 5\ 6 \end{matrix} \qquad \frac{\dot{U}\gamma}{\dot{V}} = \begin{matrix} 5\ 4\ 6 \\ 4\ 6\ 5 \\ 6\ 5\ 4 \end{matrix}$$

$$\frac{\dot{U}\delta}{\dot{V}} = \begin{matrix} 5\ 6\ 4 \\ 4\ 5\ 6 \\ 6\ 4\ 5 \end{matrix} \qquad \frac{\dot{U}\epsilon}{\dot{V}} = \begin{matrix} 4\ 6\ 5 \\ 5\ 4\ 6 \\ 6\ 5\ 4 \end{matrix}$$

It may be observed that these divisions may be effected with great rapidity; because when three out of the nine figures (in any quotient) not in the same line or column are known, all the rest are known. Thus, for example, to find $\frac{\dot{U}\epsilon}{V}$ it is only necessary to seek in $\dot{U}\epsilon$ the syntheme which contains 1.2.7, and then to take out the figure in that syntheme associated with 8.9 in that line, viz. 4; then again to seek the syntheme which con-

tains 1 . 2 . 8, and to take out the figure in that syntheme associated with 7 . 9, which is 6; and finally to seek the syntheme which contains 2 . 3 . 7, and then to take out the figure associated with 8 . 9, viz. 5; we thus obtain the three corner figures of the square which represents $\frac{\dot{U}\epsilon}{V}$ as thus:

$$\begin{matrix} 4 & 6 & . \\ 5 & \vdots & . \\ . & . & . \end{matrix}$$

of which the six remaining figures are given by the condition that in no line and in no column must the same two figures be found. In order to compare these quotients, or rather the relations of the first of them to the remaining five with those of α to $\alpha, \beta, \gamma, \delta, \epsilon$, it will be convenient to subtract the constant number 3 from each figure, and to transpose the first and second columns; we thus obtain

$$\frac{U\alpha}{V} \equiv \begin{vmatrix} 1 & 2 & 3 \\ 2 & 3 & 1 \\ 3 & 1 & 2 \end{vmatrix} \equiv \pi_1 \equiv \alpha,$$

$$\frac{\dot{U}\alpha}{\dot{V}} \equiv \begin{vmatrix} 1 & 2 & 3 \\ 2 & 3 & 1 \\ 3 & 1 & 2 \end{vmatrix} \equiv \pi_1 \equiv \alpha, \qquad \frac{\dot{U}\beta}{\dot{V}} \equiv \begin{vmatrix} 3 & 2 & 1 \\ 1 & 3 & 2 \\ 2 & 1 & 3 \end{vmatrix} \equiv \pi_9 \equiv \beta,$$

$$\frac{\dot{U}\gamma}{\dot{V}} \equiv \begin{vmatrix} 1 & 2 & 3 \\ 3 & 1 & 2 \\ 2 & 3 & 1 \end{vmatrix} \equiv \pi_{11} \equiv \delta, \qquad \frac{\dot{U}\delta}{\dot{V}} \equiv \begin{vmatrix} 3 & 2 & 1 \\ 2 & 1 & 3 \\ 1 & 3 & 2 \end{vmatrix} \equiv \pi_6 \equiv \gamma,$$

$$\frac{\dot{U}\epsilon}{\dot{V}} \equiv \begin{vmatrix} 3 & 1 & 2 \\ 1 & 2 & 3 \\ 2 & 3 & 1 \end{vmatrix} \equiv \pi_3 \equiv \epsilon.$$

Thus, for greater brevity, considering the five types to be represented by

$$\begin{matrix} \alpha & \alpha & \alpha & \alpha & \alpha \\ \alpha & \beta & \gamma & \delta & \epsilon, \end{matrix}$$

or still more briefly by

$$\alpha \quad \beta \quad \gamma \quad \delta \quad \epsilon;$$

and calling the nomes N_1, N_2, N_3, we find that the effect of interchanging N_1 and N_2 with each other is to change

$$\alpha \quad \beta \quad \gamma \quad \delta \quad \epsilon$$

into

$$\alpha \quad \beta \quad \delta \quad \gamma \quad \epsilon.$$

In like manner it may be ascertained (and the student is ad-

vised to satisfy himself by actual trial of the fact) that the effect of interchanging N_1 and N_3 with each other is to convert

$$\alpha \quad \beta \quad \gamma \quad \delta \quad \epsilon$$

into

$$\alpha \quad \delta \quad \gamma \quad \beta \quad \epsilon.$$

From these two calculations it follows that the effect of any permutation between N_1, N_2, N_3 is to produce a permutation in β, γ, δ *inter se*, but will leave α and ϵ unaltered*. Hence then we have arrived at the goal of our inquiry, having demonstrated that

$$\left| \begin{matrix} V\alpha \\ \dot{V}\alpha \end{matrix} \right|$$

indicates one type,

$$\left| \begin{matrix} V\alpha \\ \dot{V}\beta \end{matrix} \right|, \quad \left| \begin{matrix} V\alpha \\ \dot{V}\gamma \end{matrix} \right|, \quad \left| \begin{matrix} V\alpha \\ \dot{V}\delta \end{matrix} \right|$$

each of them another *the same* type, and

$$\left| \begin{matrix} V\alpha \\ \dot{V}\epsilon \end{matrix} \right|$$

a third type,—and bearing in mind that

(α)	belongs to	π_1 exclusively,
(ϵ)	,,	π_2, π_3 ,,
(β)	,,	π_7, π_8, π_9 ,,
(γ)	,,	π_4, π_5, π_6 ,,
(δ)	,,	$\pi_{10}, \pi_{11}, \pi_{12}$,,

and that each form of π comprehends 12 groupings due to the 12 forms of $V\alpha$, we are enabled to affirm that the total number of groupings of the binomial triads of 3 nomes of 3 elements each is 144, and that the number of types or species between which these 144 are distributed is 3, comprising 12, 24, and 108 respectively,—a conclusion which it would almost have exceeded the practical limits of human labour and perspicuity to have established by the direct comparison of the 144

* This result, by the aid of a fine observation, may be more rapidly established *uno ictu* (I mean by *one* calculation instead of two) as follows. Let $N_1 N_2 N_3$ be made to undergo a *cyclical* interchange, then it will be found that β, γ, δ also undergo a cyclical interchange, whilst α and ϵ remain unchanged. This proves that β, γ, δ are only different phases of the same type, which is *sufficient*; for as regards α and ϵ, the fact of the number of individuals which they represent being unequal *inter se*, and also unequal to the number contained in β, γ, δ, renders it *à priori* impossible to allow that they can either pass into each other or into the forms β, γ, δ, by virtue of any interchange among the elements.

groupings of 18 synthemes each with each other, with a view to ascertain which admit of being permutable into each other, and which not.

The largest species of 108 groupings, it may be observed, is subdivisible into 3 *varieties*, not really allotypical, of 36 each,—the characteristic of those groupings which belong to the same variety being that they permute *exclusively* into each other when the permutations of the elements are confined to perturbations of the order of the elements in the same nome or nomes, and the different nomes are subject to no interchange of elements between themselves.

Just so the species of 36 groupings of trinomial triads, treated of in my preceding paper, subdivides into 3 varieties or subfamilies characterized by a similar property.

The total number of modes of subdivision of 9 elements between 3 nomes being 280, it follows, from considerations of the same kind as stated in the May Number of the Magazine, that there exist transitive substitution-groups belonging to 9 elements of

$$\frac{\pi(9)}{280\times 12'}\quad\frac{\pi(9)}{280\times 24'}\quad\frac{\pi(9)}{280\times 108'}$$

that is, 108, 24 and 12 substitutions respectively.

Again, let us consider the question of forming the synthemes of the triads of a *single nome* of 9 elements into groupings where *every* triad shall be found without repetition. We may obtain such groupings by choosing arbitrarily any one of the 280 sets of 3 nomes into which the 9 elements may be segregated*, and then forming one syntheme with the three monomial triads (corresponding to such set so chosen), 18 synthemes (in any one of the 144 possible ways) of exclusively binomial triads, and 9 synthemes (in any one of the 40 possible ways) of exclusively trinomial triads; we shall thus obtain in all $280\times 144\times 40$, or 1,612,800 solutions of the question proposed; I mean 1,612,800 groupings, all satisfying the imposed condition, and reducible to 6 *genera*†, comprising respectively

$$4\times 12\times 280\quad 4\times 24\times 280\quad 4\times 108\times 280\quad 36\times 12\times 280$$
$$36\times 24\times 280\quad 36\times 108\times 280,$$

* 280 is also evidently the number of synthemes of triads belonging to one nome of 9 elements. In general the number of r-ads belonging to one nome of mn elements is

$$\frac{\pi(mn-1)\pi\big((m-1)n-1\big)\pi\big((m-2)n-1\big)\ldots\pi(n-1)}{\big(\pi(n-1)\big)^m\pi\big((m-1)n\big)\pi\big((m-2)n\big)\quad\ldots\quad\pi(n)}.$$

† The above *genera* must not be confounded with types or species. (In my preceding communications I may inadvertently have used the word

i. e. 13,440, 26,880, 120,960, 120,960, 241,920, 1,088,640 individual groupings. I conclude with putting a grand question, more easy to propose than to answer, viz. are these one million six hundred thousand (and upwards) groupings (classifiable under six distinct genera) all the possible modes and types of grouping which will satisfy the conditions of the question? and if not, what other mode or type of grouping can be found? Were I compelled to give an answer to this question, I would say that the balance of my mind leans to the opinion that the six types in question are the sole possible types of solution; but I do not pretend to rest this judgment upon any solid grounds of demonstration, nor to entertain it with any strong degree of assurance. It is a question which the effort to resolve cannot but react powerfully on our knowledge of the principles of tactic in general, and of the theory of substitution-groups in particular; and as such I submit it to the consideration of the rising chivalry of analysis, seeking myself meanwhile fresh fields and pastures new of meditation.

K, Woolwich Common,
June 6, 1861.

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family as coincident with type: *species* is the proper term.) The type of a total grouping in the problem referred to in the text will depend not only on the particular combination of the types of the binomial and trinomial partial groupings which give rise to these $6(=2\times3)$ genera, but also on the relative *phases* of the types so combined. The number of groupings in one type or species is always a submultiple of the number of permutations of the elements; whereas it will be seen that the number of groupings in one of the above genera greatly exceeds that number, which in the present case is only

$$1.2.3.4.5.6.7.8.9, \text{ or } 362{,}880.$$

Whatever may be the case in natural history, the nature of a type or species, as distinguished from a genus, family, or any other higher kind of aggregation of individuals, in *pure syntax* is perfectly clear and unambiguous; those groupings form a species which are commutable into one another by an interchange of elements: thus the different *phases* of the same type or species are in analogy with the different values of the same function arising out of a change in a constant parameter. If it should turn out that the above sixteen hundred thousand and odd groupings are not the sole solutions of which the question admits, then it will follow that even in this early instance we shall have an example not only of species and genera, but of distinct families of genera, for it is certain that the above six genera constitute within themselves a complete natural family. It will form an interesting subject of inquiry to ascertain how many types are included within each of the six genera belonging to this family; and be it never forgotten that to each species corresponds, and from it is, so to say, capable of being extracted or sublimated, a Cauchian substitution-group.

IX. *Chemical Notices from Foreign Journals. By* E. Atkinson, *Ph.D., F.C.S., Teacher of Physical Science in Cheltenham College.*

[Continued from vol. xxi. p. 504.]

IN the investigation of the new metal cæsium (Cs), which stands nearest potassium, Bunsen* has found that, besides cæsium, there exist sanother metal previously unknown, and which seems to resemble potassium as closely as does cæsium.

The platinum salt of cæsium is more difficultly soluble in water than that of potassium. On trying to separate the latter from the former by repeated boilings with water, in proportion as the quantity of potassium decreases, the continuous potassium spectrum between Kα and Kβ becomes fainter, and new lines appear, more especially two very intense ones in the violet between Srδ and Kβ. A limit is soon reached at which the quantity of potassium cannot be further diminished. This is the case when the sum of the atomic weights of the metals, combined with platinum and chlorine, has reached 109 (H=1). If from the platinum compound thus obtained the mixture of the hydrated oxides of potassium and cæsium is prepared, and if about the fifth part of this is converted into carbonate, absolute alcohol will extract from the dried mixture of the salts principally the hydrated oxide of cæsium. If this operation be repeated, a limit is ultimately attained at which the part dissolved in alcohol has a constant composition. This limit is reached when the atomic weight has risen from 109 to 123·4. The substance which has this enormous weight (next to gold and iodine the highest known) forms a deliquescent hydrate, as caustic as hydrate of potass. It also forms a deliquescent, strongly alkaline carbonate, of which about 10 parts are soluble in 100 parts of absolute alcohol at the ordinary temperature, and an anhydrous nitrate, which, unlike nitre, is not rhombic but is hexagonal, and by a hemihedral form is isomorphous with nitrate of soda, &c. The spectrum of the substances purified up to the atomic weight 123·4, shows the blue cæsium lines in the most brilliant lustre, but the violet lines of the unpurified mixture (of the atomic weight 109) in so feeble a degree, that a small addition of chloride of potassium, which is almost without perceptible action on the lines Csα, causes them entirely to disappear in consequence of the brightness of the ground produced by potassium. The material for this investigation, only amounting to a few grammes, was prepared from 44,000 kilogrammes of Dürckheim mineral water. On repeating the preparation from

* *Bericht der Akad. der Wissenschaften zu Berlin*, 1861.

about 150 kilogrammes of Saxon lepidolite, a product was obtained on the first treatment with bichloride of platinum which showed the violet lines between Srδ and Kβ in the most intense manner, but not a trace of the lines Csα. If this platinum double salt obtained from lepidolite had been a mixture, the blue line Csα must have been visible, together with the violet ones; for with the product obtained from the Dürckheim mineral water, on increasing the quantity of chloride of potassium the violet lines always disappear first, but the cæsium lines much later, and indeed only with a great excess of the potassium salt. Hence, besides potassium, sodium, lithium, and cæsium there must be a fifth alkali metal, which is present in small quantities in Dürckheim, Kreuznach and other similar mineral springs, but in lepidolite in larger quantity.

M. Ste.-Claire Deville and Troost, in continuation of their investigation on the reproduction of the natural minerals, have described the preparation of some sulphurets*.

Sulphuret of zinc is very easily prepared by melting together equal parts of sulphate of zinc, of fluoride of calcium, and of sulphide of barium. A fusible gangue of sulphate of baryta and of fluoride of calcium is obtained, in which are found beautiful crystals of sulphuret of zinc, either imbedded or arranged in geodes. Analysis proved them to be identical in composition with the natural blende; but they have an entirely different form. Instead of belonging to the monometric system, they crystallize in a regular double hexagonal prism, which is precisely the form of the crystals of sulphuret of cadmium. This singular observation supplies a link in the analogies of sulphur and cadmium, and establishes the dimorphism of sulphuret of zinc. It has received a timely confirmation in a discovery which Friedel has made† of the existence of a natural sulphide of zinc which crystallizes in the hexagonal system. On examining an argentiferous antimonio-sulphide of lead, he found imbedded certain crystals which had all the chemical reactions and composition of ordinary zinc blende, but was entirely different in crystalline form. The crystals consisted of a double hexagonal pyramid, with occasionally the faces of the hexagonal prism. These faces are strongly striated parallel to the base, and the angle between the adjacent faces of the pyramid was found to be about 129°, which is very near that of one of the pyramids of Greenockite. It has four easy cleavages parallel to the base and to the faces of the hexagonal prism. Its action on polarized light further establishes the crystalline form of the

* *Comptes Rendus*, May 6, 1861. † Ibid. May 13.

substance. Friedel proposes to give the name *Wurtzite* to this dimorphous variety of zinc blende.

Deville and Troost have also obtained this hexagonal blende by a kind of sublimation. Some sulphuret of zinc placed in trays in a porcelain tube, was heated to bright redness in a current of hydrogen. No hydrogen was absorbed, and no trace of sulphuretted hydrogen produced. Notwithstanding this the sulphuret of zinc appeared as if volatilized, and was removed to the cooler parts of the tube in the form of transparent crystals of the greatest regularity. Hexagonal blende had been formed, as was seen by a powerful action on polarized light. The reaction had doubtless taken place in the following manner. The sulphuret of zinc at a red heat had been reduced by hydrogen, forming a mixture of zinc vapour and sulphuretted hydrogen. Arrived slowly in the cooler parts of the tube, an inverse reaction occurred; the zinc again took up sulphur to form hexagonal blende, and hydrogen became free. It served as mineralizing agent; and the native sulphide may have been formed in the same manner. That the volatilization of zinc was only apparent, was proved by heating sulphide of zinc to a very high temperature in a current of sulphuretted hydrogen. No trace of sublimation was obtained in the porcelain tube.

A number of crystallized oxides may be obtained by heating in a platinum crucible a mixture of the sulphates of these oxides and of alkaline sulphates. The oxide thus liberated at a very high temperature in melted sulphate of potass or soda crystallizes. Debray, who had previously obtained glucina in this way, has also succeeded in preparing magnesia (periclase) and oxide of nickel *. With sulphate of manganese pretty large crystals of red oxide of manganese, $Mn^3 O^4$, were obtained, but they were so interlaced that it was impossible to measure their angles with sufficient accuracy to be enabled to identify them with Hausmannite. They have the same composition and hardness; the colour of their powder is the same, but the artificial crystals are transparent.

Alumina, magnetic oxide of iron, and green oxide of uranium may be obtained by an analogous method, based on the decomposition of certain phosphates by alkaline sulphates at a high temperature.

Gorup-Besanez † recommends the use of ozone for cleaning and restoring the colour of old spotted and soiled books and prints.

* *Comptes Rendus*, May 13, 1861.
† Liebig's *Annalen*, May 1861.

Ozone completely removes writing ink; but printing ink is not attacked by it, at any rate to no perceptible extent: grease spots and mineral colours also remain unchanged, but vegetable colours are completely removed. The method used is as follows:—The air in a sulphuric-acid carboy is ozonized by Schönbein's method, which consists in placing in it a piece of phosphorus 3 inches long and $\frac{1}{2}$ an inch thick, and pouring into the carboy as much water at 30° C. as will half cover the phosphorus; the carboy is loosely corked and allowed to stand in a moderately warm place until the air is charged with ozone, which generally requires from twelve to eighteen hours. Without removing the phosphorus and water, the article to be bleached is uniformly moistened with distilled water, and after being rolled up is suspended by a platinum wire in about the centre of the carboy. The roll of paper is soon seen to be continually surrounded by the column of vapour rising from the surface of the phosphorus. The time required for the bleaching depends on the nature of the substance, but never requires more than three days; paper brown with age and coloured with coffee spots, in two days was quite white and clean. If the paper were now dried, it would not only be very brittle, but would also rapidly become brown; hence the acid must be completely removed. The paper is immersed in water, which is frequently renewed, until it only gives a very feeble acid reaction with litmus. It is next placed in water to which a few drops of solution of soda have been added, and then, being spread on a piece of glass and placed in an inclined position, is exposed to a thin stream of water for twenty-four hours. After being allowed to stand till nearly dry, it is carefully removed and dried between blotting-paper.

Gorup-Besanez found that ozone was not well adapted for cleaning oil colours.

Pohl has communicated* a research on the white gunpowder invented by Augendre, which consists of prussiate of potash, white sugar, and chlorate of potash. Pohl finds that the following mixture gave a very good burning powder:—

Prussiate of potash	28	parts.
Sugar	23	,,
Chlorate of potash	49	,,
	100	

which is very nearly in the relation

$$K^2\,Cfy, 3HO + C^{12}H^{11}O^{11} + 3KO\,ClO^5.$$

Of the products formed by the combustion of this powder, it

* *Sitzungsberichte der Wiener Akademie*, vol. xli.

would be difficult to state any thing with accuracy without very numerous analyses, and they would differ according as the combustion was free or in a closed space, and whether it was slow or rapid. Assuming that the possible products of decomposition of the ferrocyanide are nitrogen, cyanide of potassium, and a carbide of iron of the formula FeC^2, the decomposition might be represented in the following manner:—

$$K^2\,Fe\,Cy^3 + C^{12}\,H^{11}\,O^{11} + 3\,(KO\,ClO^5) = 2\,K\,Cy + 3\,K\,Cl + Fe\,C^2 + N + 6\,CO + 6\,CO^2 + 14\,HO;$$

according to which 100 parts of the powder would yield 52·56 parts of non-volatile, and 47·44 of gaseous substances.

The decomposition may take place in conformity with other reactions, but from a preliminary investigation this appears the most probable.

In accordance with this, 100 parts of the powder would yield—

Nitrogen	1·86
Carbonic oxide	11·19
Carbonic acid	17·59
Water	16·79
	47·43

and

Cyanide of potassium	17·38
Chloride of potassium	29·84
Carbide of iron	5·33
	52·55

hence, reduced to volumes at 0° and 760 millims., 100 parts would yield—

Nitrogen	1927	cub. centims.
Carbonic oxide	8943	,,
Carbonic acid	8943	,,
Aqueous vapour	20867	,,
	40680	,,

Pohl calculates from this that the quantity of heat furnished by the combustion of this substance would be equal to 506·3 thermal units. The temperature of the combustion, is obtained by dividing the number of thermal units by the sum of the specific heats of the products of combustion, which amounts to 0·2636; and this gives 1920° C. as the temperature. From Bunsen and Schischkoff's research*, it appears that the heating effect of ordinary gunpowder is 619·5 thermal units, and that the temperature of its free combustion is 2993° C. It furnishes in

* Phil. Mag. vol. xv. p. 489.

100 parts 68·06 of solid residue, and 31·38 of gaseous products, corresponding to 19310 cubic centims. From these data the relation between the two substances is—

	Black powder.		White powder.
The quantity of gas . . .	1	:	2·107
The temperature of flame .	1	:	0·641
The residues	1	:	0·77

But for the respective temperatures of combustion the reduced volumes of gas would be for black powder 231411 cubic centims., and for white powder 300798 cubic centims., and hence the quantities of gas would be as 1 : 1·13.

In the combustion in a confined space the temperature of the combustion would be altered, for there would be a great difference in the specific heat of the products of combustion. Hence the volume of gas, when reduced to the normal temperature and pressure, would vary. For white gunpowder, Pohl calculates the temperature of combustion in a closed space at 2604° C., and the volume of gas furnished by 100 parts at 431162 cubic centims. Under similar circumstances Bunsen and Schischkoff found that the temperature was 3340° C., and the volume of gas 258420 cubic centims.

Hence the relation between the products of combustion in confined space would be—

	Black powder.		White powder.
The temperature of the flames . .	1	:	0·779
The volumes of gas	1	:	1·669

As the action of an explosive powder principally depends on the volume of the gases formed, for equal weights the new white powder would produce 1·67 times the action of the other. But for equal volumes of the powder the ratio would be different. Pohl found that a vessel which held 102·542 grms. of white powder, held 132·355 grms. of ordinary black powder. Hence the density of the new powder in reference to the other would be as 0·774 : 1, and the work performed by equal volumes would be as 1·292 : 1.

In order to produce the same effect on projectiles, in firing mines, &c., 60 parts by weight of the new, would be required for 100 parts of the old. The weights of the residues in the two cases are respectively 31·53 and 68 parts. Another advantage of the white powder is, that the temperature of the flame is much lower; a greater number of shots could be fired without heating the projectile too much. The new powder is more energetic in its action than the old, and in this respect stands nearest gun-cotton. It has the advantage over this substance of being cheaper and

easier to prepare, and it can be kept for a long time without undergoing any change.

The new powder contains chlorate of potash; and this, in all substitutes for gunpowder of which it is a constituent, forms products of combustion at a high temperature, which attack the firearms. If the decomposition of the white powder takes place in accordance with the equation already given, it is not easy to see why this evil is to be feared. It could be most simply decided by firing off a certain number of shots with a given weapon. Another advantage of the new powder is its difficult explosibility by pressure and percussion. Explosion is only produced by the heaviest blow of iron upon iron; it is not produced by the friction of wood upon metal, or between stones, &c. The new powder is also far easier of preparation than the old; and if the raw materials are at hand, a large quantity of it may be prepared in a few hours with no other apparatus than a stamping-mill and mixing tub.

The following observations have been made by Ste.-Claire Deville* on the influence exerted by the sides of certain vessels on the motion and composition of gases traversing them.

In laboratories, earthen and stoneware vessels are often used for reaction with gases at a high temperature. They are suitable for most gases; but they are permeable to hydrogen, and they absorb water.

1. A rapid current from hydrogen is passed through one of these tubes. The tube is closed by two corks, in which are fitted glass tubes; one of these tubes admits hydrogen; the other, which dips in water, serves for the escape of the gas. On closing the stopcock by which hydrogen enters, not only does the gas cease to be liberated, but the water rises to a height of 60 to 70 centimetres above its level, as if the hydrogen had been drawn into the interior of the apparatus. With coal-gas the aspiration is less, and appears to depend on the density of the gas; and there is none at all in the case of carbonic acid.

2. If the air be passed more slowly into the interior of the tube, but still more rapidly than in the majority of operations, the gas collected in the trough is no longer hydrogen, but pure air.

3. If an earthen tube be made red-hot in a furnace, and a current of hydrogen be passed through it, a mixture of carbonic acid and nitrogen (and also sulphurous acid if the combustible contains pyrites) is obtained—that is, the gases of combustion by which the tube is surrounded. The experiment succeeds

* *Comptes Rendus*, March 1861.

even when the gases in the interior are under a pressure of 7 to 8 centimetres of mercury.

4. This experiment may be made more striking by the following method:—the earthen tube is enclosed in a larger glass tube, and carbonic acid is passed into the annular space between the two tubes, while hydrogen traverses the earthen tube. The two gases emerge by two distinct delivery tubes. One of the two currents of gas is inflammable, and it is precisely that which proceeds from the end of the apparatus communicating with the source of carbonic acid. The gases change their places during this short and rapid passage.

X. *On the Principles of Energetics.* Part II. *Molecular Mechanics.* *By* J. S. Stuart Glennie, *M.A., F.R.A.S.*

[Concluded from vol. xxi. p. 358.]

31. THE misconception of my theory of material forces displayed in the remarks of Professor Challis *, seems to require a brief and, I trust, clearer restatement of the proposed first principle of Molecular Mechanics, along with its second and third principles, before proceeding to their application and development.

32. (I.) Matter is conceived as made up, not of an elastic æther and inelastic atoms, but of elastic molecules of different orders as to size and density.

If a rough physical conception of these molecules be required, they may be conceived as æthereal nuclei, the æther of the nuclei of a lower being made up of nuclei of a higher order, and so on *ad infinitum*.

I shall, perhaps, best defend this principle by restating the *experimental* objections to that for which it is substituted:—(1) We are led by experiment to conclude that *all* matter is elastic, and hence we are not justified in assuming two kinds of matter, an elastic and inelastic. (2) Not to speak of the inconsistency of denying to the atoms the elasticity which is attributed to the æther, which must be made up of atoms, it is impossible to conceive that, from any arrangement of inelastic atoms, the elasticity of the bodies they constitute should arise; though it is at once admissible that *degrees* of elasticity may depend on the arrangement of elastic atoms. (3) The action of an elastic æther, or anything else, on an "absolutely hard, ultimate atom" is experimentally inconceivable; for all known action of one body on another implies motion of the particles of that other body. If a body is struck, it is heated, and moves; if it moves little, it is

* Phil. Mag. p. 504, June 1861.

much heated. (4) The analogy "to the production of secondary waves, when a small obstacle is encountered by primary waves, propagated on the surface of water," is fallacious. For all such obstacles are elastic, and it seems a prodigious assumption to imagine that unknown inelastic, would have the same effect as known elastic bodies. (5) Not only the generation of secondary waves, but the origination of primary waves is experimentally inconceivable. For the hypothesis is—inelastic atoms of the same inertia in a *uniform* æther; and the experimental condition of motion is—*difference* of pressure.

33. (II.) Physical phenomena are to be explained from the conception of motions of different orders of molecules.

This conception has been forced on me by the impossibility of reconciling the notion of Electricity, Light, Heat, Actinism, &c. as states of molecular strain or motion, with the experimental facts of their coexistence, and mutual modification.

34. (III.) Chemical phenomena are to be explained from the conception of systems of different orders of molecules in dynamical equilibrium.

Bodies are thus conceived as systems of molecular motion; their sensible differences as dependent on differences of the orders, and motions, of their constituent molecules; their permanence as dependent on the continuance of dynamic equilibrium, that is, of the same state of motion at every point; and dissolution, combination, or the formation of new bodies as the result of difference between two or more systems of molecular motion, in mediate or immediate contact. As an illustration of this and the preceding principle, take the explanation afforded of Dr. Tyndall's discovery of the greater absorption and radiation of compound gases*. A compound gas will, in this theory, be conceived as a system, the moving molecules of which are of a relatively low order. Now it is clear that, suppose, for instance, the molecules, whose motions determine the chemical character of the gas, are of order (1), and that the molecules, whose vibrations give the sensation of heat, are of order (6), there are the motions of five, instead of, as in a simple gas, a smaller number of orders of molecules to be affected. Hence, degree of absorption of motion is seen to depend on the number of motions to be affected.

But I must reserve a fuller explanation of this and connected phenomena to its proper place in the development of the theory; for my object in these papers has merely been to give a general introductory statement and explanation of the Principles of Energetics, forming the basis of Ordinary Mecha-

* Bakerian Lecture, 1860.

nics (Stereatics and Hydratics) and Molecular Mechanics (Physics and Chemics).

35. As to Analytical Investigations on the basis of these principles, there does not seem to be any peculiar difficulty raised when any separate order of molecules is considered. But that there will be found great analytical difficulty in passing from a higher to a lower order of molecules, and in expressing the correlations of their motions, is not to be concealed. And that little help is to be found in existing analytical investigations, except, perhaps, those relative to the conduction of heat in crystalline media, is not to be wondered at, seeing how recent is the conception of the Correlation of Forces.

6 Stone Buildings, Lincoln's Inn,
25th June, 1861.

XI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xxi. p. 536.]

November 22, 1860.—Major-General Sabine, R.A., Treasurer and Vice-President, in the Chair.

THE following communications were read :—

"On Boric Ethide." By Edward Frankland, Ph.D., F.R.S., and B. Duppa, Esq. Received July 7, 1860.

When zincethyle in excess is brought into contact with tribasic boracic ether, $\left(B\begin{cases}C_4H_5O_2\\C_4H_5O_2\\C_4H_5O_2\end{cases}\right)$, the temperature of the mixture gradually rises for about half an hour. If it be now submitted to distillation, it begins to boil at 94° C., and between this temperature and 140° a considerable quantity of a colourless liquid distils over. The distillation then suddenly stops, the thermometer rises rapidly, and, to avoid secondary products of decomposition, the operation should now be interrupted. The materials remaining in the retort solidify, on cooling, into a mass of large crystals, which are a compound of ethylate of zinc with zincethyle. On rectification, the distillate began to boil at 70°, but the thermometer rapidly rose to 95°, at which temperature the last two-thirds of the liquid passed over and were received apart. The product thus collected exhibited a constant boiling-point on redistillation. Submitted to analysis, it yielded results agreeing with the formula

$$B\begin{cases}C_4H_5\\C_4H_5\\C_4H_5\end{cases}$$

This body, for which we propose the name *boric ethide*, is produced by the following reaction :—

$$\underbrace{2B\left\{\begin{matrix}C_4H_5O_2\\C_4H_5O_2\\C_4H_5O_2\end{matrix}\right.}_{\text{Boracic ether.}}+\underbrace{3Zn_2\left\{\begin{matrix}C_4H_5\\C_4H_5\end{matrix}\right.}_{\text{Zincethyle.}}=\underbrace{2B\left\{\begin{matrix}C_4H_5\\C_4H_5\\C_4H_5\end{matrix}\right.}_{\text{Boric ethide.}}+\underbrace{6\left\{\begin{matrix}C_4H_5\\Zn\end{matrix}\right\}O_2}_{\text{Ethylate of zinc.}}$$

The ethylate of zinc thus formed combines with zincethyle to form the crystalline compound above alluded to.

Boric ethide possesses the following properties:—It is a colourless mobile liquid of a pungent odour; its vapour is very irritating to the mucous membrane, and provokes a copious flow of tears. The specific gravity of boric ethide at 23° C. is ·6961; it boils at 95° C., and the results of several determinations of its vapour-density give the number 3·4006. The calculated vapour-density of boric ethide, volumetrically composed like terchloride of boron, is 3·3824.

Boric ethide is insoluble in water, and is very slowly decomposed by prolonged contact with it. Iodine has scarcely any action upon it, even at 100° C. It floats upon concentrated nitric acid for several minutes without change; but suddenly a violent oxidation takes place, and crystals of boracic acid separate. When boric ethide vapour comes in contact with the air it produces slight bluish-white fumes, which have a high temperature. The liquid is spontaneously inflammable in air, burning with a beautiful green and somewhat fuliginous flame. In contact with pure oxygen it explodes. Placed in a flask and allowed to oxidize gradually, first in dry air and finally in dry oxygen, it forms a colourless liquid, which boils at a higher temperature than boric ethide, but cannot be distilled under atmospheric pressure without partial decomposition. In a stream of dry carbonic acid this product of oxidation evaporates without residue. By distillation *in vacuo* it is obtained pure, and it then exhibits a composition expressed by the formula

$$B\left\{\begin{matrix}C_4H_5\\C_4H_5O_2\\C_4H_5O_2\end{matrix}\right.$$

The product of the oxidation of boric ethide is therefore the *diethylate* of a body which may be conveniently named *boric dioxyethide*, $\left(B\left\{\begin{matrix}C_4H_5\\O\\O\end{matrix}\right.\right)$. The formation of diethylate of boric dioxyethide from boric ethide may be thus represented:

$$\underbrace{B\left\{\begin{matrix}C_4H_5\\C_4H_5\\C_4H_5\end{matrix}\right.}_{\text{Boric ethide.}}+O_4=\underbrace{B\left\{\begin{matrix}C_4H_5\\C_4H_5O_2\\C_4H_5O_2\end{matrix}\right.}_{\text{Diethylate of boric dioxyethide.}}$$

This compound dissolves instantly in water, and is resolved into alcohol and a volatile white crystalline body, which may be sublimed without change, at a gentle heat, in a stream of carbonic acid, and then condenses in magnificent crystalline plates like naphthaline.

The analytical results yielded by this body agree closely with the formula

$$B\left\{\begin{matrix}C_4H_5\\H\,O_2\\H\,O_2\end{matrix}\right.$$

It is therefore obviously produced by the substitution of two atoms of hydrogen for two of ethyle in diethylate of boric dioxyethide:

$$\underbrace{B\left\{\begin{matrix}C_4H_5\\C_4H_5O_2\\C_4H_5O_2\end{matrix}\right.}_{\text{Diethylate of boric dioxyethide.}}+2\left\{\begin{matrix}H\\H\end{matrix}\right\}O_2=\underbrace{B\left\{\begin{matrix}C_4H_5\\HO_2\\HO_2\end{matrix}\right.}_{\text{Dihydrate of boric dioxyethide.}}+2\underbrace{\left\{\begin{matrix}C_4H_5\\H\end{matrix}\right\}O_2}_{\text{Alcohol.}}.$$

Dihydrate of boric dioxyethide possesses an agreeable etherial odour and a most intensely sweet taste. Exposed to the air it evaporates slowly at ordinary temperatures, undergoing at the same time partial decomposition, and invariably leaving a slight residue of boracic acid. Its vapour tastes intensely sweet. It reddens litmus paper, although in other respects its acid qualities are very obscure. It is very soluble in water, alcohol, and ether. Exposed to a gentle heat it fuses, and at a higher temperature boils with partial decomposition.

We are at present engaged with the further study of these bodies, and with the corresponding reactions of zincethyle upon silicic, carbonic, and oxalic ethers.

"On Fermat's Theorems of the Polygonal Numbers." First Communication. By the Right Hon. Sir Frederick Pollock, F.R.S., Lord Chief Baron. Received July 11, 1860.

"On Cyanide of Ethylene and Succinic Acid." Preliminary Notice. By Maxwell Simpson, Ph.D. Received August 1, 1860.

Succinic acid bears the same relation to the diatomic alcohol (glycol) that propionic acid bears to ordinary alcohol. Propionic acid can be obtained by treating the cyanide of the alcohol radical with potash. Can succinic acid be obtained by treating the cyanide of the glycol radical with the same reagent, or is it an isomeric acid that is formed under these circumstances?

$$\underbrace{C_4H_5,\,Cy}_{\text{Cyanide of ethyle.}}+O_2\left\{\begin{matrix}K\\H\end{matrix}\right.+2HO=O_2\left\{\begin{matrix}C_6H_5O_2\\K\end{matrix}\right.+NH_3.$$

Propionate of potash.

$$\underbrace{C_4H_4\,2Cy}_{\text{Cyanide of ethylene.}}+2\left(O_2\left\{\begin{matrix}K\\H\end{matrix}\right.\right)+4HO=O_4\left\{\begin{matrix}C_8H_4O_4''\\K_2\end{matrix}\right.+2NH_3.$$

Succinate of potash?

The following experiments were performed with the view of determining this point:—

Preparation of Cyanide of Ethylene.—As a preliminary step to

the formation of succinic acid in this way, it became of course necessary to prepare the cyanide of ethylene. This body I obtained by submitting bromide of ethylene to the action of cyanide of potassium.

The process was thus conducted:—A mixture of two equivalents of the cyanide and one of the bromide was introduced into a large balloon, together with a considerable quantity of alcohol, sp. gr. ·840, and exposed to the temperature of a water-bath, a Liebig's condenser having been previously attached to the balloon in such a manner as to prevent the alcohol from distilling off the reacting ingredients. As soon as all the cyanide of potassium had been converted into bromide, the alcohol was separated and distilled. A semifluid residue was thus obtained, which was filtered at the temperature of 100° Cent. On treating the filtrate with a saturated solution of chloride of calcium, a reddish oil rose to the surface, which was well washed with ether, and exposed for some time to the temperature of 140°, in order to remove any bromide of ethylene that might have escaped the solvent action of the ether. This body proved, on analysis, to be cyanide of ethylene. It was not, however, quite pure. There are difficulties attending its complete purification which I have not yet overcome.

At the temperature of the air, cyanide of ethylene is a semisolid crystalline mass of a brownish colour. It melts under 50° Cent. It is very soluble in water and alcohol, and sparingly soluble in ether. It cannot be distilled. Nevertheless it bears a tolerably high temperature without suffering much decomposition. Heated with an alcoholic solution of potash, it gives off ammonia. Treated with nitric acid, it forms a body which crystallizes from alcohol in long needles. This and some other reactions I am at present engaged in studying.

Preparation of Succinic Acid.—Bromide of ethylene and cyanide of potassium were made to react upon each other in the same manner as in the preparation of the cyanide of ethylene. As soon as the reaction was complete, the alcohol was separated from the bromide of potassium, some sticks of caustic potash were added to it, and the whole heated for several days by means of a water-bath. Torrents of ammonia were given off on applying the heat. As soon as the evolution of this gas had ceased, the alcohol was distilled off and the residue treated with a considerable excess of hydrochloric acid. This was then heated gently as long as acid vapours continued to be evolved, digested with absolute alcohol, and filtered, and then the filtrate was evaporated to dryness. The dry mass thus obtained was treated several times with alcohol in a similar manner. The result of these repeated digestions was then dissolved in water, and a few drops of a solution of nitrate of silver were added to it, which occasioned a slight precipitate of chloride of silver. This was separated by filtration, and the filtrate was exactly neutralized with ammonia. On adding excess of nitrate of silver to this, an abundant white precipitate was obtained, very soluble in nitric acid and ammonia. This gave, on analysis, numbers agreeing very well with the composition of succinate of silver. The acid itself possessed also all the properties of succinic acid. It sublimed

on the application of heat, was soluble in water, alcohol, and ether, and gave, when neutralized, a reddish-brown precipitate with perchloride of iron. Moreover, on digesting this precipitate with ammonia, an acid could be detected in the filtered liquor, which gave white precipitates with nitrate of silver, and with a mixture of chloride of barium and alcohol.

Succinic acid *can* then be obtained from glycol in the same manner as propionic acid from ordinary alcohol; the bromide of ethylene, the point from which I started, being capable of derivation from the diatomic alcohol.

I propose extending this investigation to some other hydrocarbons of the series $C_n H_n$, with the view of ascertaining whether or not the homologues of succinic acid can be obtained from these bodies by a similar process.

"Results of Researches on the Electric Function of the Torpedo." By Professor Carlo Matteucci of Pisa. In a Letter to Dr. Sharpey, Sec. R.S. Received August 3, 1860.

"It has hitherto been believed that the action of the electric organ of the Torpedo was momentary only;—that it becomes charged, under the influence of nervous action and discharged immediately that action ceases, somewhat like soft iron under the influence of an electric current. Such, however, is not the real state of the case. The electric organ is always charged. It may be conclusively shown by experiment that the action of that organ never ceases, and that round the body of a Torpedo, and probably of every other electric fish, there is a continual circulation of electricity in the liquid medium in which the animal is immersed. In fact, when the electric organ, or even a fragment of it, is removed from the living fish and placed between the ends of a galvanometer, the needle remains deflected at a constant angle for twenty or thirty hours, or even longer.

"I must here explain that in electro-physiological experiments it is highly advantageous to employ, as extremities of the galvanometer, plates of amalgamated zinc immersed in a neutral saturated solution of sulphate of zinc. This arrangement, which can be worked with the greatest facility, gives a perfectly homogeneous circuit, leaving the needle at zero in an instrument of 24,000 coils; the liquid in contact with the animal part experimented on has the greatest possible conductibility while it does not act chemically on the tissue, and the apparatus is entirely free from secondary polarity.

"To return to the Torpedo. The electric organ, or a portion of it, detached from the fish and kept at the temperature of freezing, preserves its electromotive properties for four, six, or even eight days; and an organ which has been kept for twenty-four hours in a vessel surrounded with a frigorific mixture of ice and salt, is found to possess an electromotive power as great as that of the organ recently detached from the living fish. Thus the electric organ retains its functional activity long after both muscular and nervous excitability have been extinguished.

"What then is the action of the nerves on this apparatus? Here again experiment affords a very distinct and conclusive answer. De-

tach the organ of a live torpedo and cut it into two equal portions, in such a way as to leave each half in connexion with one of the large nervous trunks; place the two halves on a plate of gutta percha, with electric couples opposed; that is, with the similar surfaces (say the dorsal) in contact; and connect the two free (ventral) surfaces with the extremities of the galvanometer. There will usually be no deflection of the needle, or, at most, a very slight effect which will soon disappear. Now, after having opened the circuit of the galvanometer, irritate the nerve of one of the segments, by pinching, by the interrupted electric current, or in any other way; or prick the piece itself with a needle. The portion of organ thus stimulated will give several discharges in succession, and a rheoscopic frog's limb with its nerve applied to the part will each time be thrown into violent convulsions. If, after this, the galvanometer be applied as before, there will be a very strong deflection in a direction answering to the segment stimulated. This deviation endures for a short time, but gradually becomes less, so that in a few minutes the effect of the two segments is equal. Stimulation now of the other segment will in like manner render its electricity predominant. These alternations may be repeated several times, but naturally the effect becomes less and less marked.

"Thus the electromotive apparatus becomes charged and acts independently of the influence of the nerves, but that influence renews and renders persistent the activity of the apparatus. We know, moreover, that the discharge, which is only a state of temporary increased activity of the organ, is brought on by an act of the will in the live animal, or by the excitation of the nerves of the organ.

"I shall not enter now into further details respecting my recent experiments on the Torpedo, but I venture to think that we have really made a step towards clearing up the theory of the animal electromotive apparatus. The organ of the Torpedo does not, under the influence of the nerves, act as an induction apparatus; the operation seems more analogous to that of a 'secondary pile,' created, through the influence of the nerves, in each constituent cell of the organ.

"The case is very different in muscular action, the changes occurring in which are better understood now that we know the phenomena of muscular respiration. I do not here refer to the variation of the muscular current which takes place at the moment of contraction. In that case it would appear from experiment, as I lately showed, that there are indications of a current in an opposite direction; but the conditions of the animal structure in action are so complex that no inference can be drawn as to the intimate nature of the phenomenon. It is otherwise, however, in comparing muscles which have been left at rest with muscles which have been fatigued by repeated contraction. Being still engaged in the investigation of this matter, I shall content myself now with mentioning one result of my inquiry, which I consider as well established; the result, in fact, of performing on muscles the same kind of experiment as the one above described on the organ of the Torpedo. The experiment is as follows:—Having selected a series of muscles, entire or divided,

which have been proved (by my method of opposed muscular piles) to be equal in electromotive power ; subject a certain number of them to repeated stimulation, and then, by means of the method of opposed couples, compare the muscles which have been exercised with those which have been left at rest, and it will be found that the latter will manifest a much greater degree of electromotive power than the former. The nervous excitation, which causes muscular contraction, developes heat, generates mechanical force and consumes chemical affinity; and as the electromotive apparatus of muscle operates through means of that affinity, it must get weakened, like a pile in which the acid has become weaker. In the Torpedo, on the other hand, there is neither heat nor mechanical force produced, and the electromotive apparatus is set up again, as it were, through the influence of the nerves, after the manner of a secondary pile."

" Natural History of the Purple of the Ancients." By M. Lucaze Duthiers, Professor of Zoology in the Faculty of Sciences of Lille. Received March 22, 1860.

" Contributions towards the History of Azobenzol and Benzidine." By P. W. Hofmann, Ph.D. Received July 24, 1860.

Among the numerous compounds into which benzol, when submitted to reagents, is converted, *azobenzol* and its derivatives have as yet received but limited attention. Although more than twenty-five years have elapsed since this interesting body was discovered by Mitscherlich, both its formation and its constitution remain still doubtful.

Mitscherlich*, who discovered azobenzol in 1834, when submitting nitrobenzol to the action of an alcoholic solution of potassa, represented this compound by the formula

$$C_6 H_5 N \dagger,$$

but left the reaction which gives rise to the formation of azobenzol unexplained. In 1845 this body was reprepared by Hofmann and Muspratt‡, who observed among the collateral products of the reaction *aniline* and *oxalic acid*. They represent the formation of azobenzol by the equation

$$\underbrace{2C_6 H_5 NO_2}_{\text{Nitrobenzol.}} + \underbrace{C_2 H_6 O}_{\text{Alcohol.}} = \underbrace{C_6 H_5 N}_{\text{Azobenzol.}} + \underbrace{C_6 H_7 N}_{\text{Phenylamine.}} + \underbrace{C_2 H_2 O_4}_{\text{Oxalic acid.}} + H_2 O,$$

adding at the same time that they are far from considering this equation as more than the representation of *one* phase of the transformation of nitrobenzol, since several other rather indefinite compounds or products are formed simultaneously.

At about the same period Zinin made the interesting observation that azobenzol is capable of fixing hydrogen and of being thereby

* Pogg. Ann. xxxii. p. 224.
† H = 1, O = 16, C = 12, &c.
‡ Mem. of the Chem. Soc. vol. iii. p. 113.

converted into a well-defined base, benzidine, which he represented by the formula

$$C_6 H_6 N.$$

Considering the physical characters both of azobenzol and of benzidine, especially the high boiling-points of these substances, and the ratio of hydrogen and nitrogen in the latter compound, the sum of the number of equivalents of these two elements not being divisible by 2, many chemists were inclined to double the formulæ of both bodies, and to represent them by the following expressions:—

Azobenzol..... $C_{12} H_{10} N_2$
Benzidine..... $C_{12} H_{12} N_2$

This view received the first experimental confirmation in the formation of the nitro-derivatives of azobenzol, which were examined in 1849 by Gerhardt and Laurent. The formation of

Nitrazobenzol.... $C_{12} H_9 N_3 O_2 = C_{12} (H_9 \ NO_2) \ N_2$, of
Dinitrazobenzol.. $C_{12} H_8 N_4 O_4 = C_{12} \left[H_8 (NO_2)_2\right] N_2$,

and of several derivatives of these bodies, having established the C_{12}-formula of azobenzol, but little doubt could be entertained regarding the formula of benzidine, which is as readily obtained from azobenzol by reducing agents, as it may be reconverted into azobenzol by nitric acid*.

The molecular value of benzidine being thus almost exclusively fixed by the determination of the formula of the compound from which it originates, it was of some interest to obtain additional experimental evidence for the molecular weight of azobenzol.

With this view I have determined the vapour-density of azobenzol. This body boiling at a rather high temperature, I have availed myself of the method of displacement lately proposed by Professor Hofmann. Experiment proved the density of the azobenzol-vapour to be 94 referred to hydrogen as unity, or 6·50 referred to air. The theoretical vapour-density of azobenzol, assuming that one molecule of this compound furnishes, like the rest of well-examined substances, 2 vols. of vapour†, is $\frac{182}{2} = 91$ referred to hydrogen, and 6·32 referred to air.

The determination of the vapour-density, then, plainly confirms the higher molecular weights proposed for azobenzol and for benzidine.

When determining the vapour-density of azobenzol, I had occasion to observe that, probably in consequence of a typographical error, the boiling-point of this compound is misstated in all the manuals which I could consult, and even in the original memoirs of Mitscherlich himself. The boiling-point is stated to be 193° C., whilst it is in reality 293° C.

Benzidine, when expressed by the formula

$$C_{12} H_{12} N_2,$$

* Noble, Journal of the Chem. Soc. vol. viii. p. 293. † $H_2 O = 2$ vols.

presents itself as a well-defined diacid diamine. The molecular construction of the diatomic base remained to be decided.

I have endeavoured to solve this problem by the process of ethylation, as yet the simplest and the best guide in determining questions of this kind. Benzidine in the presence of alcohol is rapidly attacked by iodide of ethyle. After two hours' digestion at 100° C. in sealed tubes, the reaction is complete. The solution on evaporation yields a crystalline iodide,

$$C_{16}H_{22}N_2I_2=C_{12}H_{12}(C_2H_5)_2N_2I_2,$$

from which ammonia separates a solid crystalline base very similar to benzidine. This compound, which fuses at 65° C., and resolidifies at 60° C., is *diethylbenzidine*:

$$C_{16}H_{20}N_2=C_{12}H_{10}(C_2H_5)_2N_2,$$

which forms well-crystallizable salts with the acids, and yields with dichloride of platinum a difficultly soluble crystalline platinum-salt containing

$$C_{16}H_{22}N_2Cl_2\,2PtCl_2.$$

When diethylbenzidine is treated again with iodide of ethyle, the phenomena previously observed repeat themselves. The iodide

$$C_{20}H_{30}N_2I_2=C_{12}H_{10}(C_2H_5)_4N_2I_2$$

is formed, which when decomposed by ammonia yields *tetrethylbenzidine*

$$C_{20}H_{28}N_2=C_{12}H_8(C_2H_5)_4N_2.$$

Tetrethylbenzidine resembles the diethylated and the non-ethylated base. It fuses at 85° C., resolidifying at 80° C., produces with the acids crystalline compounds, and furnishes with dichloride of platinum a platinum-salt of the formula

$$C_{20}H_{30}N_2Cl_2,\,2PtCl_2.$$

The further action of iodide of ethyle upon tetrethylbenzidine is extremely slow. After 12 hours' digestion at 100° C. only a very minute quantity of the base had been transformed into an iodide. Iodide of methyle, on the other hand, acts with greate nergy. An hour's digestion is sufficient to produce the final diammonium-compound.

The iodide

$$C_{22}H_{34}N_2I_2=C_{12}H_8(C_2H_5)_4(CH_3)_2N_2I_2$$

is very difficultly soluble in absolute alcohol, but dissolves with facility in boiling water, from which it is deposited on cooling, in long beautiful needles. The solution of this iodide is no longer precipitated by ammonia, but yields with oxide of silver a powerfully alkaline solution, exhibiting all the characters of the completely substituted ammonium- and diammonium-bases discovered by Professor Hofmann. The solution of this dimethyl-tetrethylated base, which contains

$$C_{22}H_{36}N_2O_2=\left.\begin{matrix}C_{12}H_8(C_2H_5)_4(CH_3)_2N_2\\H_2\end{matrix}\right\}O_2,$$

is not further acted upon by either iodide of ethyle or methyle. With acids it forms a series of salts which are remarkable for the beauty with which they crystallize. The platinum-salt is almost insoluble in water, but soluble with difficulty in concentrated boiling hydrochloric acid, crystallizing from this solution on cooling in beautiful needles. This salt contains

$$C_{22}H_{34}N_2Cl_2, 2PtCl_2.$$

The above experiments appear to establish the molecular construction of benzidine in a satisfactory manner. This base is obviously a primary diamine, in which the molecular group $C_{12}H_8$, whatever its nature may be, functions as a diatomic radical. A glance at the subjoined Table exhibits the construction of benzidine and of the several compounds which I have described.

Diamines.

Benzidine.....	$(C_{12}H_8)''$ H_2 H_2	N_2,
Diethylated benzidine	$(C_{12}H_8)''$ $(C_2H_5)_2$ $(H_2)_2$	N_2,
Tetrethylated benzidine	$(C_{12}H_8)''$ $(C_2H_5)_2$ $(C_2H_5)_2$	N_2.

Iodides of Diammoniums.

Primary	$[(C_{12}H_8)''$	$H_6 \quad N_2]''I_2$,
Secondary	$[(C_{12}H_8)''$	$H_4 \quad (C_2H_5)_2N_2]''I_2$,
Tertiary	$[(C_{12}H_8)''$	$H_2 \quad (C_2H_5)_4N_2]''I_2$,
Quartary.....	$[(C_{12}H_8)''$	$(CH_3)_2 \quad (C_2H_5)_4N_2]''I_2$.

"On Bromphenylamine and Chlorphenylamine." By E. T. Mills. Received July 24, 1860.

Nitrophenylamine, when prepared from dinitrobenzol (*i. e.* by the indirect method), differs in so many respects from the isomeric base which is obtained from phenyle-compounds (*i. e.* by the direct method), that chemists have distinguished these two bodies as alpha- and beta-nitrophenylamine*—Bromphenylamine and chlorphenylamine

* The alpha-nitrophenylamine (nitraniline) was formed about sixteen years ago by Dr. Muspratt and myself (Chem. Soc. Mem. vol. iii. p. 112), by the action of reducing agents on dinitrobenzol. The beta-nitraniline was discovered by Arppe (Chem. Soc. vol. viii. p. 175), who obtained this compound when distilling pyrotartronitrophenylamide with potash. The two bases resemble each other in a remarkable manner; but there are differences in their physical and chemical characters which leave no doubt as to the fact of their having different constitutions. I may here remark that I have repeated Arppe's experiments, the results of which I can confirm in every particular. Since the phenyle-compound from which Arppe obtained his substance is accessible only with difficulty, I have endeavoured to nitronate a more easily procurable phenyle-compound. Acetyl-

have hitherto been produced only by the action of potash upon bromisatine and chorisatine, the indirect method, by which they were originally obtained by Dr. Hofmann; it appeared therefore of some interest to ascertain whether the bodies generated directly from compounds of phenylamine would exhibit differences in their properties similar to those which distinguish alpha- and beta-nitrophenylamine.

With the view of deciding this question experimentally, I have submitted acetylphenylamide to the action of bromine and chlorine, in the hope of thus forming directly from phenylamine the brominated and chlorinated compounds in question.

Action of Bromine on Acetylphenylamide.

A cold aqueous solution of acetylphenylamide, when agitated with bromine gradually added in small quantities until the yellow colour imparted to the liquid no longer disappears, furnishes a crystalline compound difficultly soluble in cold, but easily recrystallizable from boiling water. The substance consists chiefly of monobrominated acetylphenylamide

$$(C_8 H_8 Br N O = \left.\begin{matrix}(C_6 H_4 Br)\\ (C_2 H_3 O)\\ H\end{matrix}\right\} N,$$

which is however invariably mixed with small quantities of dibrominated acetylphenylamide

$$(C_8 H_7 Br_2 N O = \left.\begin{matrix}(C_6 H_3 Br_2)\\ (C_2 H_3 O)\\ H\end{matrix}\right\} N.$$

I have not been able to find a method of separating these two bodies perfectly.

The brominated compound is readily attacked by potash. On distilling the mixture, the vapour of water carries over a volatile

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phenylamide may be used for this purpose with considerable advantage. A solution of the compound in cold fuming nitric acid yields, on the addition of water, a crystalline difficultly soluble precipitate, which is easily obtained pure by recrystallization. This substance contains

$$C_8 H_8 N_2 O_4 = \left.\begin{matrix}[C_6 (H_4 NO_2)]\\ (C_2 H_3 O_2)\\ H\end{matrix}\right\} N,$$

and yields, when heated with potassa, the beta-nitrophenylamine of Arppe with all its properties. I may here recall a former observation, which has now become perfectly intelligible. When studying the action of nitric acid upon melaniline, I found (Chem. Soc. Mem. t. i. 305) that the dinitromelaniline, which is thus formed, essentially differs from the dinitromelaniline obtained by submitting nitrophenylamine (alpha-) to the action of chloride of cyanogen. The two nitrobases, which are both expressed by the formula

$$C_{13} H_{11} N_5 O_4 = C_{13} [H_{11} (NO_2)_2] N_3,$$

stand to each other in the same relation which obtains between alpha-nitrophenylamine and beta-nitrophenylamine. In fact, I have since found that the distillation of the nitro-base, obtained by treating alpha-nitrophenylamine with chloride of cyanogen, furnishes alpha-nitrophenylamine; whilst beta-nitrophenylamine may be detected amongst the products of the distillation of the dinitromelaniline which is formed directly from melaniline by means of nitric acid.—A. W. H.

body which solidifies in the condenser into beautiful acicular crystals, acetate of potassium remaining in the retort.

The solidified distillate was purified by recrystallization from boiling water, and submitted to analysis. Both the combustion of the base itself and the platinum-determination of the beautiful golden-yellow platinum-salt proved this body to be bromphenylamine

$$C_6H_6BrN = \left.\begin{matrix}(C_6H_4Br)\\H\\H\end{matrix}\right\}N.$$

In its appearance, odour, and taste, as likewise in its deportment with acids and with solvents generally, the brominated base obtained from acetylbromophenylamide resembles perfectly the bromphenylamine produced from bromisatine, a specimen of which I obtained from Dr. Hofmann's collection. There is only one point in which a slight difference was observed. Both compounds are capable of crystallizing either in needles or in well-defined octahedra, the former being generally obtained from water, and the latter from alcohol. The bromphenylamine, obtained from the acetyle-compound, appears to be more inclined to crystallize in needles than in octahedra. Circumstances have prevented me from entering into an examination of the products of decomposition of the two bromphenylamines; and the question whether these two bodies are really identical, or similarly related as the two nitro-compounds, must be decided by further experiments*.

Action of Chlorine on Acetylphenylamide.

The phenomena observed in the action of chlorine on a cold saturated solution of the phenyle-compound are perfectly similar to those presented in the corresponding reaction with bromine. A crystalline compound immediately separates from the solution; as soon as the crystals cease to augment, the current of chlorine is interrupted. Washed with cold, and once recrystallized from boiling water, the chlorinated body is found to be nearly perfectly pure monochlorinated acetylphenylamide

$$C_8H_8ClNO = \left.\begin{matrix}C_6H_4Cl\\C_2H_3O\\H\end{matrix}\right\}N,$$

which, when distilled with potash, furnishes abundance of chlorphenylamine, resembling in a marked manner the chlorphenylamine obtained by the action of potash upon chlorisatine.

"New Compounds produced by the substitution of Nitrogen for Hydrogen." By P. Griess, Esq. Received July 24, 1860.

In several previous notes I have called attention to a peculiar double acid which is formed by the action of nitrous acid upon amidobenzoic acid,

$$C_{14}H_{14}N_2O_4 + HNO_2 = C_{14}H_{11}N_3O_4 + 2H_2O\dagger,$$

* These experiments have since been made by Mr. P. Griess, whose results are given in the next abstract.—A. W. H.

† H=1; O=16; C=12, &c.

the constitution of which, as far as my experiments go, may be represented by the formula

$$\left.\begin{matrix}[C_7(H_3N_2')O][C_7(H_4,H_2N)O]\\ H_2\end{matrix}\right\} O_2.$$

There are not less than three other compounds known which empirically may be represented by the same formula as amidobenzoic acid, viz. nitrotoluol, salicylamide, and anthranilic acid. The two former substances differ from amidobenzoic acid both physically and chemically in a marked manner; anthranilic acid, on the other hand, is so closely allied to the benzoic derivative, that special experiments were required to distinguish these two bodies. Gerland, when he submitted the two acids to Piria's well-known reaction, observed that both are converted by nitrous acid into non-nitrogenated acids, which, although still isomeric, essentially differ in their properties; amidobenzoic acid being transformed into a new acid,—oxybenzoic acid, whilst anthranilic acid yields salicylic acid.

It appeared of some interest to try whether the substitution of nitrogen for hydrogen in anthranilic acid would furnish a compound isomeric with the double acid obtained from amidobenzoic acid. A current of nitrous acid, when passed into a cold alcoholic solution of anthranilic acid, rapidly transforms this substance into a compound crystallizing in white prisms, which is easily obtained by allowing the alcohol to evaporate at the common temperature. The new body is extremely soluble in water, insoluble in ether. By analysis it was proved to contain

$$C_{14}H_9N_5O_7.$$

The new compound is thus seen to be far from isomeric with the derivative of amidobenzoic acid produced under similar circumstances, with which, in fact, it shows no analogy whatever. I have not yet arrived at a definite view regarding the molecular construction of this body; nevertheless its deportment with water shows even now that the nitrogen in it exists in two different forms. Gently heated with water, the new compound disengages torrents of nitrogen; on cooling, the liquid solidifies into a crystalline mass of salicylic acid, free nitric acid remaining in solution. This metamorphosis is represented by the equation

$$\underbrace{C_{14}H_9N_5O_7}_{\text{New body.}}+2H_2O=N_4+HNO_3+\underbrace{2C_7H_6O_3}_{\text{Salicylic acid.}},$$

which has been controlled by quantitative experiments. The idea suggests itself to assume one-fifth of the nitrogen in the form of nitric acid, when the new body might be viewed as a salt-like compound of the formula

$$\left.\begin{matrix}C_7H_4N_2'O_2\\ C_7H_4N_2'O_2\end{matrix}\right\} HNO_3;$$

the action of the water consisting simply in the replacement of the monatomic nitrogen by the elements of water, which would produce salicylic acid, nitric acid being liberated.

I avail myself of this opportunity of mentioning the deportment of

several other isomeric bodies under the influence of nitrous acid. There are two basic compounds,

$$C_6 (H_5 NO_2) N,$$

known; the one is the alphaphenylamine of Hofmann and Muspratt, the other the betaphenylamine observed by Arppe. When submitted to the action of nitrous acid, these two isomeric bodies yield two perfectly different nitrogen-substituted derivatives. The substance obtained from alphaphenylamine (the base formed by the reduction of dinitrobenzol) has been already mentioned in one of my previous notes, the body derived from betaphenylamine is still under examination.

The action of nitrous acid proves that there are also two bromphenylamines similar to the two nitrophenylamines. The original bromphenylamine discovered by Hofmann, and which is formed by the distillation of bromisatine with hydrated potash, yields with nitrous acid a compound,

$$C_{12} H_9 Br_2 N_3 = \left.\begin{matrix} (C_6 H_4 Br)_2 \\ N''' \\ H \end{matrix}\right\} N_2,$$

crystallizing in beautiful golden-yellow needles, insoluble in water, and difficultly soluble in alcohol and ether. The bromphenylamine, on the other hand, which was lately prepared by Mills* from acetylbromphenylamide, exhibits with nitrous acid a perfectly different deportment, being transformed into a yellow scarcely crystalline compound, easily soluble in alcohol and ether, but insoluble in water. I have not as yet analysed this compound; its formation, however, and its properties render it probable that it will be found to be isomeric with the product of decomposition previously mentioned. I am engaged in a more minute examination of this compound, which I hope may assist in explaining the cause of the still enigmatical isomerism exhibited by the derivatives of phenylamine.

I have already repeatedly called attention to the different atomicity exhibited by nitrogen under different conditions. In the derivatives of amidobenzoic and of anthranilic acids, it can be proved that 1 equiv. of nitrogen replaces 1 equiv. of hydrogen; while in the derivatives of phenylamine, the nitrogen is present with the value of three molecules of hydrogen.

GEOLOGICAL SOCIETY.

[Continued from vol. xxi. p. 539.]

April 24, 1861.—Leonard Horner, Esq., President, in the Chair.

The following communications were read:—

1. "On the 'Symon Fault' in the Coalbrook Dale Coal-field." By Marcus W. T. Scott, Esq., F.G.S.

This communication was based on observations made during many years on a section through a part of the Shropshire Coal-field in

* See the previous abstract.

nearly a straight line from north to south, commencing at the Greyhound Pit, near Oakengates Tunnel of the Shrewsbury and Birmingham Railway, and terminating at John Anstice and Co.'s Halesfield Pits near Madely. Particular reference was made to the explanation of the nature of the Great East or Symon Fault. The author commenced making his observations on the Malinslee and Stirchlee Royalties in 1843; and in 1845 he came to the conclusion that what the miners termed in this locality the "Symon Fault," that is the successive dying out of certain coal-seams, ironstones, &c. at various depths underground, was due to an old denudation which had produced an inclined surface at the expense of some of the beds before the upper measures were deposited. Having obtained, in course of time, correct sections of several pits situated in the N.–S. line above mentioned, the author, taking the "Little Flint" (the lowest workable coal) as a base-line, plotted the several shifted segments of the coal-field in a vertical plan, and thus restored the original outline of the denuded area (one side of a valley) as seen in a transverse section. Six sinkings in the N.–S. line having indicated the successive disappearance of five workable coal-beds in a distance of 2484 yards, a seventh pit, 2000 yards further south, was found to yield all the coals again; and the author thinks that between the 6th (the Grange) and the 7th (Halesfield) pit the coals re-occur successively on the opposite side of the old valley of denudation, and that they may here be sought for and worked advantageously. The line of the old valley of denudation apparently strikes the Great East fault, as laid down on the Geological Survey Map, at a considerable angle.

2. "On the Occurrence of *Cyrena fluminalis* associated with Marine Shells in Sand and Gravel above the Boulder-clay at Kelsey Hill near Hull." By Joseph Prestwich, Esq., F.R.S., Treas.G.S. &c.

The author's observations tended to show that the *Cyrena fluminalis*, instead of being limited, in its occurrence, to beds beneath the Boulder-clay (under which circumstance it is found in Norfolk), occurs in deposits of newer date, and that the argument, that the well-known beds at Grays, in Essex, are older than the Boulder-clay, depending much on the presence of this shell, would lose much of its force if this *Cyrena* were proved to belong also to the newer geological horizon. The question is now the more important, as this shell has been found by Mr. Prestwich in the beds that contain flint implements at Abbeville.

The author proceeded to show that some gravels and sands near Hull in Yorkshire, formerly described by Professor Phillips, contain abundance of the *Cyrena fluminalis*, associated with twenty-two species of marine shells, two of which have Arctic characters, the others being common littoral forms. These gravels and sands were proved, by well-sections and other exposures, especially by borings and trenches made by the author and Mr. T. J. Smith, of Hull, to overlie the Boulder-clay.

XII. *Intelligence and Miscellaneous Articles.*

ON THE SOLIDIFICATION OF CERTAIN SUBSTANCES.

BY M. L. DUFOUR.

IN a preceding communication it has been shown that water, kept in suspension in a liquid of its own density, could be cooled much below 0° without solidifying. It was probable that other bodies placed in similar conditions would experience a similar retardation of solidification. The following are three examples:—

Sulphur.—The persistence of this body in the fluid state below 115° has been already noted (M. Person and Prof. Faraday), but it is an exception not frequently mentioned.

It is easy to prepare a solution of chloride of zinc which has the same or a little higher density than that of liquid sulphur. This solution can be heated to 115° without boiling; sulphur may be melted in it, and then floats in spheres. In order to keep the spheres surrounded by liquid, a layer of oil may be poured on the solution. On cooling, the solidification scarcely ever takes place at the melting-point. The liquid globules usually sink to 70°, 50°, &c. before solidifying. The solidification is spontaneous, or it may be provoked by the contact of a solid body, especially of a fragment of sulphur; but in the special conditions of these experiments the liquid state presents a remarkable stability. At 60°, salts, metallic wires, &c. may occasionally be introduced into globules 6 millims. in diameter without inducing an immediate solidification. Globules of $\frac{1}{2}$ a millim. in diameter frequently remain fluid at 5°, and persist in that state for several days.

When the spheres of sulphur remain liquid at 50° or 60° below the ordinary temperature of solidification, it is truly interesting to see their change of state. The fluid mass, which is transparent and of a deep red, suddenly changes into a hard, opake yellow fragment. This experiment, which is very pretty and easily performed, is exceedingly well adapted to exhibit the curious phenomenon of superfusion.

Phosphorus.—M. Desains has already noted the conservation of the liquid state by this body below 44°. The method which serves for sulphur is perfectly applicable to phosphorus. The solution of chloride of zinc of a suitable density is covered with a layer of oil in order to avoid contact of the air. The liquid transparent globules of phosphorus are easily seen, and their solidification does not take place till far below 44°. Globules, $\frac{1}{2}$ to 2 millims. in diameter, frequently sink to 5° or even to 0°. The liquid state is also remarkably stable, and the change of condition gives occasion for observations analogous to those relative to sulphur.

Naphthaline.—The fusion and solidification of this body usually take place at 79°. It has almost exactly the same density as water, but is somewhat less dense in the liquid state. With suitable precautions the phenomenon of superfusion may be easily produced. It is merely requisite to melt the body in a flask filled to the neck with boiled water, and then to incline the flask so that the liquid napthaline lodges in the upper part of the flask, pressed, but feebly

so, against the side of the glass. In virtue of the slight difference in density, this liquid assumes the spheroidal form, and does not adhere to the glass. I have seen globules 8 millims. in diameter retain the liquid state to 55°.

It is probable that other bodies, if placed in suitable conditions, would also present the phenomenon which the preceding bodies manifest in such a pronounced degree. Unfortunately it is difficult with a large number to realize the essential condition, which is to pass the ordinary temperature of change of state while the body floats in equilibrium in a liquid of the same density. The liquid selected must, in point of fact, fulfil the four following conditions: it must have the same density as the body under experiment, retain the fluid state above and below its melting-point, and not exert any chemical action. Spite of these requirements, I do not doubt that chemistry will furnish the means of successfully applying to other substances the method by which the retardation of the solidification of water, sulphur, and phosphorus is so easily and certainly effected.—*Comptes Rendus*, April 29, 1861.

ON THE CHANGES PRODUCED IN THE POSITION OF THE FIXED LINES IN THE SPECTRUM OF HYPONITRIC ACID BY CHANGES IN DENSITY. BY M. WEISS.

Weiss has found by actual measurement that the distance between the dark lines in the spectrum of hyponitric acid diminishes as the density of the gas increases. The measurements were made with an Oertling's circle reading directly to two seconds of arc, and, by a filar micrometer in the ocular, to a single second. The same phenomenon occurs with the spectrum of chlorophyll. The stronger the extract in ether, the less is the distance of the absorption-bands. Thus the absorption-band in the red, in the case of a strong extract, corresponds quite well with Fraunhofer's line C; in the case of a weak extract it stands at some distance from it. The other absorption-bands in this spectrum undergo similar dislocations.

These changes in the distances of the dark lines are very sensible, even in the spectrum of hyponitric acid, when the changes in the density of the gas are considerable; they are not, however, equal for all the dark lines.

The cause of these dislocations is to be sought, according to Weiss, in a one-sided absorption which each line undergoes toward the violet end of the spectrum when the density of the body is increased. This is shown by direct observations and comparisons with the solar spectrum as well as by numerous measurements. There is no specific absorption upon both sides of each line, but only an absorption upon the side of the line which lies toward the violet end of the spectrum. In this manner the bands become broader, and the distance between them less. The author has observed similar changes in the breadth of Fraunhofer's lines at sunset. In this case also the absorption was only upon one side. From this it appears that the lines of hyponitric acid cannot be used as standards in determinations of indices, &c.—Poggendorff's *Annalen*, vol. cxii. p. 153, Jan. 1861; and Silliman's *Journal* for May.

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[FOURTH SERIES.]

AUGUST 1861.

XIII. *On the Klaprothine or Lazulite of North Carolina.* By E. J. CHAPMAN, *Professor of Mineralogy and Geology in University College, Toronto**.

THE Klaprothine or lazulite is comparatively a rare mineral. It appears to have been first recognized by Widenmann in 1791, in the valley of the Muhr, near Krieglach in Upper Styria. By Werner it was mistaken for felspar; and, although examined by Klaproth, its true nature was not detected until the analysis by Fuchs of specimens afterwards discovered near Werzen in Salzburg. Brandes then examined the Krieglach specimens, and showed their identity in composition with the examples analysed by Fuchs†. The other known localities of this mineral comprise Vorau near Gratz in Styria (examples from which spot have been analysed by Rammelsberg); the foot of the Wechsels near Therenberg in Lower Austria; Minas Geraes in Brazil; and Sinclair County in North Carolina. Specimens from this latter locality have been very carefully analysed by Professor J. Lawrence Smith and George J. Brush (now Professor of Metallurgy in Yale College); but I have failed to discover in any publication a crystallographic or mineralogical description of this North American lazulite. A specimen, however, consisting of numerous small crystals imbedded in fine granular quartz or sandstone, having been kindly presented to me within the few last months, by

* Communicated by the Author.

† Brandes appears, however, to have missed the water present in this substance, unless there be a typographical error in his recorded numbers. If we transpose these numbers, as regards the silica (an impurity) and the half per cent. of water said to have been obtained, his analysis will agree closely with those of other chemists.

Prof. T. Sterry Hunt, of the Geological Survey of Canada, I propose in the present place to offer a brief notice of its leading mineralogical characters.

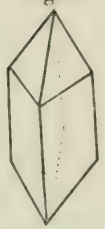
All the earlier determinations of lazulite crystals referred the mineral to the trimetric or rhombic system. Prüfer of Vienna was the first to maintain its monoclinic character; and the angles given in the more recent works on mineralogy are adopted from his measurements. The European crystals present in general a somewhat complicated aspect, although certain combinations closely resemble those of the trimetric system. Two "augite pairs" are always present. These, according to Prüfer, measure respectively over a front edge 100° 20′ and 99° 40′, the difference being but little more than half a degree. According to the same observer, moreover, the inclination of the base on the prism-plane (0 P : ∞ P, in the notation of Naumann) only differs from a right angle by 23′. Were these values consequently all that we had to depend upon, it would be manifestly unsafe to rely upon them as proofs of the monoclinic crystallization of lazulite. But in some combinations the forms below the middle zone of the crystal are less numerous than those above this zone, or otherwise differ from the latter in their measurements. Nevertheless in certain trimetric minerals, and notably in datolite and Wolfram, we have the same peculiarity, and we might therefore look upon these lazulite crystals as trimetric combinations hemihedrally modified. From my examination of the North Carolina specimens, I cannot but think that this view will in the end prevail. It is supported by the fact that in many combinations the upper and lower forms do actually correspond in number and character, and that practised crystallographers like Phillips and Lévy, skilled in the use of the goniometer, were unable to detect in their measurements the differences announced by Prüfer*.

The North Carolina crystals (presuming those in my possession to represent the generality of crystals obtained at this locality), although usually distorted, are of extreme simplicity, contrasting remarkably in this respect with the majority of European examples. At first sight they resemble a monoclinic prism terminated by a single "augite pair" or hemi-pyramid; but they really consist (if monoclinic) of two hemi-pyramids, the four planes of one of which are greatly elongated; or if trimetric (as I conceive them to be), they form a rhombic octahedron in which four planes, in opposite sets of two, are thus lengthened beyond

* These observers appear to be the only crystallographers who have practically examined crystals of lazulite. Thus the measurements of Phillips are followed by Hausmann, Breithaupt, and others; those of Lévy, by Dufrénoy; and those of Prüfer, by Naumann, Dana, Quenstedt, and Miller.

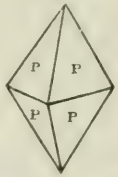
the others. Fig. 1 represents this distorted aspect; fig. 2 the same form (or combination, if monoclinic) in symmetrical proportions. These symmetrical crystals are of smaller size and less numerous than the distorted forms.

Fig. 1.



Although the edges of these crystals are sharply defined, the planes are unfortunately without lustre. The most careful measurements of five crystals, by means of a fixed or Adelmann's goniometer, gave me the same angles for both the upper and lower faces. The difference found by Prüfer is too slight, however, to be satisfactorily detected by any kind of application goniometer. I attached, therefore, thin films of mica as carefully as possible to the planes of one of the crystals, and measured the angles by reflected light with a Wollaston goniometer of the best construction. The following Table (sustaining the apparently trimetric character of these crystals) shows the measurements thus obtained:—

Fig. 2.



	Upper planes over front edge.	Lower planes over front edge.
1st measurement . . .	100° 4′	100° 0′
2nd „ . . .	99 99	99 99
3rd „ . . .	99 99	100 2

	Upper planes over side edge.	Lower planes over side edge.
1st measurement . . .	97° 27′	97° 28′
2nd „ . . .	97 30	97 26
3rd „ . . .	97 26	97 27

	Front planes over middle edge.	Back planes over middle edge.	
1st measurement	134° 10′	134° 7′	Whether monoclinic or trimetric, these measurements should of course correspond. The two sets were taken, however, for greater satisfaction.
2nd „	134 10	134 12	
3rd „	134 8	134 10	

Adelmann's goniometer gave me 100°—100° 30′ over a front edge, 97°—97° 30′ over a side edge, and 134°—134° 30′ over a middle edge. If we look upon the mineral as trimetric, and adopt the angle of 100° as the mean inclination over a front edge, with 91° 30′ for the value of the prism-angle (according to general adoption), the following angles and axial relations are obtained by calculation:—

P : P (over a front edge)	$=100^\circ\ 0'$
P : P (over a side edge)	$=97\ 24\frac{1}{2}$
P : P (over a middle edge)	$=134\ 12$

x (vertical axis)	$=1{\cdot}652$
$\bar{x}$ (macrodiagonal)	$=1$
$\breve{x}$ (brachydiagonal)	$=0{\cdot}9741$

The measurements of Phillips give for the octahedral angles, as deduced by Hausmann, 99° 15′ (over front edge), 96° 39′ (over side edge), and 136° 20′ (over middle edge). The position of the crystals, as adopted by Phillips, is here changed, however, his middle edge being made a front polar edge, and the reverse.

Many of these North Carolina crystals appear to possess another form in addition to those enumerated above. This is the front polar or macrodome, occurring generally on two opposite edges only, and thus presenting a monoclinic character, but lying sometimes on only one edge, and being consequently (if the mineral be trimetric) a tetartohedral modification. It is a mere line, dull like the other planes, and too narrow to admit of satisfactory measurement. The crystals are sometimes implanted in one another; but I have not detected any definite twin-combinations. The crystals extracted from my specimen, together with those exposed on the surface of this, do not amount, however, to more than ten or twelve in number. The hardness of these crystals is equal to 5·75, or very nearly to 6·0. The specific gravity (one determination only) I found to equal 3·108, a value corresponding sufficiently with that obtained by Smith and Brush (3·122). The cleavage I have not been able to determine in a satisfactory manner. The blowpipe reactions are as follows:—

In the closed tube the assay gives off water and loses its colour, becoming yellowish or greyish white.

Per se, it exfoliates and expands greatly in bulk, changes colour, tinges the flame green, and crumbles away without fusing.

In borax it dissolves very easily, imparting to the glass a pale ferruginous tinge.

In salt of phosphorus it dissolves also very readily, and with slight effervescence.

In carbonate of soda it dissolves partially, but the dissolved portion is in great part precipitated as the glass cools, forming a white enamel. If the bead be dissolved in a little boiling water, a drop of nitric acid added to decompose the excess of carbonate of soda, and the clear supernatant liquid be then poured upon a

small crystal of nitrate of silver, a yellow precipitate of phosphate is at once obtained. In employing this test for phosphates, the beginner should be cautioned, however, that silicates (if decomposable by carbonate of soda) will produce the same reaction, but the silica may be eliminated by adding several drops of acid and evaporating to dryness. By treatment with salt of phosphorus, moreover, silicates are at once recognized. If the solution of our mineral, as obtained above, be treated with acetate of lead, the precipitate presents the well-known blowpipe reaction of phosphate of lead, *i. e.* the formation of a faceted globule without reduction.

Two analyses of the North Carolina lazulite are given by Professors Smith and Brush in the 'American Journal of Science and Arts' for September 1853. These exhibit the following results:—

	I.	II.
Phosphoric acid	43·38	44·15
Alumina	31·22	32·17
Protoxide of iron	8·29	8·05
Magnesia	10·06	10·02
Water	5·68	5·50
Silica (an impurity) . . .	1·07	1·07
	99·70	100·96

From the above values, Messrs. Smith and Brush have deduced the annexed formula:—

$$2[3(MgO, FeO), PO^5] + 5Al^2O^3, 3PO^5 + 5HO.$$

The true position of lazulite, in a natural classification, appears to be amongst a group of phosphates containing both anhydrous and hydrous species (the distinction between these being entirely artificial), and in some of which fluorine is also present. In this group I would place the following minerals:—Childrenite, Wavellite, Fischerite, turquoise, lazulite, Wagnerite, Herderite, amblygonite, monazite, xenotime, and cryptolite.

Toronto, Canada,
June 20, 1861.

XIV. *On the Propagation of Heat in Gases.* By G. Magnus.

[Concluded from p. 12.]

Passage of the Rays of Heat through Gases.*

An objection might still be raised against any conductibility in gases. It might be maintained that the stronger heating of the thermometer in hydrogen depended on the fact that it permits the passage of heat-rays more easily than all other gases.

* Read before the Academy of Berlin, February 7, 1861.

The above experiments with cotton and eider-down speak against this; for it can scarcely be assumed that the heating takes place through radiation. Moreover, the experiments which Dr. Franz has published * on the radiation through hydrogen, show that more heat-rays do not pass through hydrogen than through atmospheric air. It appeared, however, necessary, before maintaining that gases can conduct heat, to determine by new experiments how far that doubt was founded. Hence the determinations on the passage of heat-rays through various gases were concluded when I communicated to the Academy, in July of last year (1860), the investigation on the conduction of heat.

As far as I know, Dr. Franz's are the only experiments which have hitherto been published on the diathermancy of the gases. These, which moreover only refer to atmospheric air, hydrogen, and carbonic acid, could not be sufficient for the present purpose, because an argand lamp was used as a source of heat. But it was not merely possible, but even probable that the transmission of thermal rays would differ with the source whence they came. If therefore the experiments were to be conclusive, the transmission must be investigated for rays proceeding from the same source of heat, that of boiling water.

Boiling water as a source of Heat.

Dr. Franz in his experiments enclosed the gases in tubes closed at both ends by pieces of plate glass. Now from Melloni's experiments †, rays from so low a source of heat penetrate plate glass in scarcely perceptible quantities. Even when I used plates of rock-salt to close a tube a metre in length, the action which the rays of boiling water produced upon the thermo-pile were so small, that a comparison of the different gases furnished no satisfactory results. It further appeared desirable to avoid any kind of plates, even of rock-salt; for although, from Melloni's experiments, the rays which have passed through this substance comport themselves exactly like those which proceed directly from the source of heat and have only passed through air, yet the rock-salt might possibly alter the rays, and exert an influence on their subsequent passage through different gases. I have accordingly undertaken new experiments on the diathermancy of gases for obscure heat, in which it was my object to allow the rays of heat to pass through the gases without necessitating their passage through any plate.

When these experiments were finished, I saw from the 'Pro-

* Poggendorff's *Annalen*, vol. xciv. p. 337.
† Ibid. vol. xxxv. p. 393.

ceedings of the Royal Society*' that Dr. Tyndall in London was engaged with an investigation on the transmission of heat through gases. As Dr. Tyndall, whose research is only just announced, has experimented with tubes which were closed by plates of rock-salt, I considered that the following investigation was independent of that of Dr. Tyndall.

The apparatus which I used was constructed as follows. Upon the plate of an air-pump T T, fig. 2. Plate I., which could be placed apart from the pump on a separate foot, a thermo-electric pile was firmly fixed by means of a cork ring cemented on the plate. The mounting of this pile was of brass, and had an internal diameter of 24 millims. and a length of 118 millims. The pile itself was only 30 millims. long. It contained 56 pairs of antimony and bismuth, which together formed a section of 30 millims. square. The wires from this pile to the galvanometer passed through the plate insulated at L L. Over the pile was a glass vessel, F G, with a broad ground edge air-tight upon the plate of the pump. This was 175 millims. high and 100 millims. in diameter.

At the upper part there were two apertures, q and r, to which corresponded the two tubes $q\,q_1$ and $r\,r_1$ of the brass cover G G, which was fastened on the top of this vessel. These tubes were 30 millims. in height. In the tube $q\,q_1$ right over the thermo-pile S, the glass vessel A B, upon which the vessel C was fused, was firmly fitted by means of a cork, and made air-tight by means of caoutchouc. In the tubulure D of this vessel there was a cork, through which a glass tube passed, which could be closed with a stopcock H. The tube $r\,r_1$ contained a stuffing-box, through which the round brass rod $a\,b$ passed. Inside the vessel F G, this rod was provided with a horizontal arm $a\,c$, at whose end c, two circular pieces of tinplate 34 millims. in diameter were fitted parallel above one another at a distance of 3 millims. They served as a screen, and when the thermo-pile was to be exposed, could be easily moved aside. This could be effected by a horizontal arm $b\,d$, fitted at $b\,b$ on the brass rod $a\,b$ outside the vessel F G. In order to protect the pile as far as possible from the influence of external sources of heat, the vessel F G was surrounded by a wide glass cylinder N M, which with its broad edge was pressed on the plate. The space between both vessels was filled up to N M with water, which was kept at a temperature of 15° C.

The vessel C contained boiling water, which by passing steam into it was kept at a temperature of 100°; and this formed the source of heat. Its action upon the thermo-pile was indicated

* Proceedings of the Royal Society, vol. x. p. 37. Phil. Mag. vol. xix. p. 60.

by means of a very delicate multiplier with a double needle, which was connected by copper wires with the binding screws L L. The wire of the multiplier consisted of copper which had been galvanoplastically deposited, and was free from iron. It is the multiplier which I used in my investigation on thermo-electric currents. I tried to use in its stead a multiplier with a steel mirror, which was read by means of a telescope and scale; but spite of this mode of observation I found it less delicate, manifestly because the steel mirror was not astatic. Whether a multiplier with mirror and astatic needle would not be better for these observations I have not tried; but I doubt it; for the reading off by means of a mirror is only suited for small differences of angles, while in using an astatic needle greater deviations are observed. The multiplier used was placed upon a firm stand separate from the rest of the apparatus.

In investigating the diathermancy of a gas, water at 100° C. was poured into the vessel C, and kept at that temperature by means of steam passed into it from a flask in the neighbourhood. The moment the screen *c c e e* was displaced, the needle began to move slowly, and after it had reached its greatest deflection, it assumed a fixed position in the course of about two minutes, after a few very small oscillations. This was read off partly directly, and partly by means of a telescope; in the latter case a rectangular prism was placed directly over that part of the divided circle which was to be observed, so that the position of the needle could be seen by reflexion. When this was effected, the screen was replaced over the pile, upon which the needle reverted to its original position. It was never, however, exactly over the null-point of the scale, either because the torsion of the wire had changed a little, or because there had been a slight difference of temperature in the pile. As in the multiplier used the coils of wire were quite free from magnetism, the replacement at 0° could easily have been effected by turning the divided circle independently of the magnetic needle. But this might easily have produced fluctuations of the entire apparatus, and it therefore appeared better to take the mean of the positions of equilibrium before and after the deflection, and to subtract this from the observed deflection. The observations thus obtained agreed very well with each other when several were made successively. It was only after the experiments had been continued some time that the numbers somewhat disagreed, because the pile became a little warmer at one end. Four to six of such observations were always made in succession, and the mean of these taken.

The values corresponding to the deflections of the galvanometer were determined by the method given by Melloni in his

'Thermochrose,' page 59. As far as 14°·5 the strengths of the current were proportional to the deviations.

In order to fill the apparatus with any given gas, it was first exhausted, the gas admitted by the stopcock H, then again exhausted and filled a second time with the gas, and so on for four times, upon which the atmospheric air could be considered to be completely removed. With gases like cyanogen and ammonia which attack the pump, the filling was performed by displacement, the gas being admitted by the stopcock C, and escaping by the stopcock K, under the plate T T. For this purpose the whole apparatus, with the plate T T, was removed from the air-pump and placed on a tripod. On the lower part of the stopcock K a glass tube was fitted, through which the gas passed into an absorbent liquid. If the gas was to be used in a rarefied state, the rarefaction was effected, after the filling was complete, by means of the air-pump. The rarefaction thus produced was either directly observed by the barometer, or a manometer was introduced, which was read off by means of a cathetometer. Thus the most different gases could be examined as to their capacity of transmitting heat, with the exception of those which attacked the metal of the pile. This excluded, to my great regret, all coloured gases.

The gases were prepared exactly as in the experiments on conduction.

As the intensity of the galvanometer might have changed in the course of time, in almost every case before the apparatus was filled with a new gas the radiation through atmospheric air was determined. In this way the relation of the radiation in the particular gas to that of atmospheric air was obtained. This method of comparison I have always retained, for it ensures great certainty. In experiments with boiling water this comparison was ultimately found superfluous, for the galvanometer remained so unchanged that the values obtained at different times for atmospheric air agreed very closely. Nevertheless in the following Table the observed deflections are so arranged that the control experiments with atmospheric air are found in one column, and the gases examined either directly before or after are placed opposite, being separated from the rest by a horizontal line.

For the gases which exhibit the greatest deviation, the radiation has been determined at different times. Since the numbers obtained agreed as closely as could be expected with such experiments and with such angles, I have only adduced one series.

Transmission of Heat by a Glass surface at 100° C.

Position of equilibrium of the needle.	Mean.	Observed deflection.	Difference.
Atmospheric air under 1 atm. pressure.			
−1°	−0°5	14°0	14°5
0	+0·2	14·5	14·3
+0·5	+0·5	15·0	14·5
0·5	+0·5	15·0	14·5
0·5			
	Mean		14·5
Atmospheric air under 1 atm. pressure			
+3·5	+2·5	17·0	14·5
1·5	1·7	16·0	14·3
2·0	2·0	16·2	14·2
2·0	2·0	16·5	14·5
2·0	2·0	16·5	14·5
2·0			
	Mean.....		14·4
Atmospheric air under 1 atm. pressure.			
−0·0	+0·25	14·7	14·5
+0·5	0·5	15·0	14·5
+0·5	0·5	15·0	14·5
+0·5	0·5	15·0	14·5
+0·5			
	Mean.....		14·5
Atmospheric air under 1 atm. pressure.			
+0·5	+0·7	15·2	14·5
+1·0	+1·1	15·5	14·4
1·2	1·2	15·7	14·5
1·2	1·5	16·0	14·5
1·7	1·5	16·0	14·5
1·2			
	Mean.....		14·5

Position of equilibrium of the needle.	Mean.	Observed deflection.	Difference.
Atmos. air under 22·5 millims. presssure.			
+0°5	+0°5	16°0	15°5
0·5	0·6	16·0	15·4
0·7	0·7	16·2	15·5
0·7	0·7	16·2	15·5
0·7			
	Mean.....		15·5
Atmospheric air under 18·0 millims. pres.			
−0·5	−0·15	14·7	14·85
+0·2	+0·7	15·7	15·0
+1·2	1·1	16·2	15·1
+1·0	1·0	16·2	15·2
+1·0	1·0	16·2	15·2
+1·0			
	Mean.....		15·1
Atmospheric air under 8 millims. pres.			
0·0	+0·1	16·0	15·9
+0·2	0·35	16·0	15·65
+0·5	0·6	16·5	15·9
+0·7	0·7	16·5	15·8
+0·7			
	Mean.....		15·8
Atmospheric air under 9 millims. pres.			
0·0	0·0	15·7	15·7
0·0	−0·1	15·7	15·8
−0·2	−0·2	15·7	15·9
−0·2	−0·2	15·5	15·7
−0·2			
	Mean.....		15·8
Hydrogen under 1 atm. pressure.			
+1·5	+1·2	15·0	13·8
+1·0	1·1	15·0	13·9
1·2	1·2	15·0	13·8
1·2	1·0	15·0	14·0
0·7	0·6	14·7	14·1
0·5			
	Mean.....		13·9

Transmission of Heat by a Glass surface (*continued*).

Position of equilibrium of the needle.	Mean.	Observed deflection.	Difference.

Position of equilibrium of the needle.	Mean.	Observed deflection.	Difference.
Atmospheric air under 1 atm. pressure.			
+2°·0	+2°·1	16°·5	14°·4
+2·2	2·3	17·0	14·7
2·7	2·6	17·0	14·4
2·5	2·3	16·7	14·4
2·2	2·2	16·7	14·5
2·2			
	Mean		14·4
Atmospheric air under 1 atm. pressure.			
0·0	+0·4	14·7	14·3
+0·7	0·6	15·0	14·4
+0·5	0·6	15·0	14·4
+0·7	0·6	15·0	14·4
+0·5			
	Mean		14·4
Atmospheric air under 1 atm. pressure.			
0·0	+0·1	14·5	14·4
+0·2	0·2	14·7	14·5
0·2	0·3	14·7	14·4
0·5	0·5	15·0	14·5
0·5			
	Mean		14·45
Atmospheric air under 1 atm. pressure.			
0·0	+0·1	14·5	14·4
+0·2	0·1	14·5	14·4
0·0	0·0	14·4	14·4
0·0	0·0	14·4	14·4
0·0			
	Mean		14·4

Position of equilibrium of the needle.	Mean.	Observed deflection.	Difference.
Protoxide of nitrogen under 1 atm. pres.			
+3°·0	+3°·2	15°·0	11°·8
3·5	3·6	15·5	11·9
3·7	3·7	15·7	12·0
3·7	3·7	15·7	12·0
3·5	3·6	15·7	12·1
3·5	3·5	15·5	12·0
	Mean		12·0
Olefiant gas under 1 atm. pressure.			
+1·2	+1·2	8·5	7·3
1·2	1·0	8·5	7·5
0·7	0·7	8·2	7·5
0·7	0·7	8·2	7·5
0·7	0·7	8·5	7·8
0·7			
	Mean		7·5
Carbonic oxide under 1 atm. pressure.			
0·0	+0·1	12·7	12·6
+0·2	0·1	13·0	12·9
0·0	0·0	13·0	13·0
0·0	−0·1	12·7	12·8
−0·2			
	Mean		12·8
Carbonic oxide under 9 millims. pres.			
+0·5	+0·25	15·5	15·25
0·0	−0·25	15·2	15·45
−0·5	−0·5	15·2	15·70
−0·5	−0·5	15·0	15·50
−0·5			
	Mean		15·5
Oxygen under 1 atm. pressure.			
0·0	+0·1	14·5	14·4
+0·2	0·2	14·5	14·3
0·2	0·35	14·7	14·35
0·5	0·5	15·0	14·5
0·5			
	Mean		14·4
Cyanogen under 1 atm. pressure.			
0·0	+0·25	12·0	11·75
+0·5	0·35	12·0	11·65
+0·2	0·35	12·0	11·65
0·5	0·50	12·2	11·70
0·5			
	Mean		11·7

If the deflections obtained with atmospheric air under a pressure of 1 atmosphere are collated, we obtain

14°·5, 14°·4, 14°·5, 14°·5, 14°·3, 14°·5, 14°·4, 14°·4, 14°·4, 14·45, 14°·4,

the mean being 14°·4.

For all other gases the deflections are less. But as the deflections are proportional to the intensities of the current up to 14°·4, and as these are proportional to the increase in temperature of the pile, the quantities of heat which pass through different gases under the same pressure are as follows:—

Atmospheric air . .	14·4	or 100
Oxygen	14·4	„ 100
Hydrogen	13·9	„ 96·5
Carbonic acid . . .	13·0	„ 90·3
Carbonic oxide . . .	12·8	„ 88·8
Protoxide of nitrogen .	12·0	„ 83·3
Marsh-gas	11·7	„ 81·2
Cyanogen	11·7	„ 81·2
Olefiant gas . . .	7·5	„ 52·1
Ammonia	6·3	„ 43·7

As oxygen gave exactly the same value as atmospheric air, it was unnecessary to examine nitrogen.

For atmospheric air the deflections were for a pressure of—

8 millims. = 15°·8
9 millims. = 15 ·8

This deflection is no longer proportional to the intensity of the current, but corresponds to a value of 16·2, the value for 1° between 0° and 14° being placed equal to 1. If it be assumed that the radiation through vacuum would produce the same deflection, the heat which passes through vacuum would be to that which passes through atmospheric air under a pressure of 1 atmosphere, as

16·2 : 14·4 = 100 : 88·88.

In order to obtain greater certainty for this proportion, I determined the radiation through rarefied air by interposing in the conduction a wire which offered considerable resistance, in order that the deflections might be smaller, and proportional to the intensities of the current. In the three following determinations the air was under a pressure of 4 millims. Directly before and after each of them the radiation through atmospheric air under a pressure of one atmosphere was determined. The corresponding determinations are indicated by the same number.

No.	Position of equilibrium of the needle.	Mean.	Observed deflection.	Difference.
Atmospheric air under 1 atm. pressure.				
I	+0°·2	+0°·35	11°·2	10°·85
	+0·5	+0·5	11·7	11·2
	+0·5	+0·25	11·2	10·95
	0·0	0·0	11·0	11·0
	0·0	0·0	11·0	11·0
	0·0			
	Mean			11·0
Ia	0°·0	−0°·1	12°·0	12°·1
	−0·25	−0·1	12·0	12·1
	0·0	0·0	12·0	12·1
	0·0			
	Mean			12·1

Ia : I = 12·1 : 11·0 = 100 : 90·9.

No.	Position of equilibrium of the needle.	Mean.	Observed deflection.	Difference.
II	0·0	+0·1	11·0	10·9
	+0·25	+0·1	11·0	10·9
	0·0	0·0	11·0	11·0
	0·0	+0·1	11·0	10·9
	+0·25			
	Mean			10·9
IIa	0·0	−0·1	12·0	12·1
	−0·25	−0·25	12·25	12·5
	−0·25	−0·1	12·5	12·6
	0·0	0·0	12·5	12·5
	0·0			
	Mean.....			12·4

IIa : II = 12·4 : 10·9 = 100 : 87·9.

No.	Position of equilibrium of the needle.	Mean.	Observed deflection.	Difference.
III	0·0	0·0	11·0	11·0
	0·0	0·0	11·0	11·0
	0·0	0·0	11·0	11·0
	0·0	0·0	12·0	12·0
	0·0			
	Mean.....			11·2
IIIa	−1·75	−1·6	10·5	12·1
	−1·5	−1·5	11·25	12·75
	−1·5	−1·75	11·0	12·75
	−2·0	−1·75	11·0	12·75
	−1·5	−1·5	11·0	12·5
	−1·5			
	Mean			12·5

IIIa : III = 12·5 : 11·2 = 100 : 89·5.

After the observations I*a* and II*a* were complete, the air was still under a pressure of 10 millims.; after III*a* it was under 7 millims.

The relation thus ascertained between radiation through rarefied air to that through atmospheric air under a pressure of 1 atmosphere, agrees so far with that previously given, that I have taken the former as a basis for calculating the relation of the radiation through other gases to that through vacuum. Hence of 100 rays which pass through vacuum, the following quantities pass through the different gases, all under the pressure of one atmosphere:—

	Deflection.		Rays.
Vacuum	15°8		
corresponding to	16·2	=	100
Atmospheric air	14·4		88·88
Oxygen	14·4		88·88
Hydrogen	13·9		85·79
Carbonic acid	13·0		80·23
Carbonic oxide	12·8		79·01
Protoxide of nitrogen	12·0		74·06
Marsh-gas	11·7		72·21
Cyanogen	11·7		72·21
Olefiant gas	7·5		46·29
Ammonia	6·3		38·88

Although these values cannot be looked upon as quite reliable, inasmuch as variations may occur from imperfect purity of the gas, or from other almost unavoidable impurities, they yet show how considerable are the differences which perfectly transparent gases exhibit in reference to the property of transmitting heat. This surprising deportment, which I had already established before I laid the first part of this treatise "On the Conduction of Heat" before the Academy, led me to make a separate investigation of the radiation through gases, and first of all to ascertain whether similar differences prevailed when another source of heat was used.

A Gas-flame as source of Heat.

I desired first of all to use a source of heat at a higher temperature, for which purpose the apparatus depicted in fig. 2, Plate I. was unfitted. I was accordingly compelled to use the gases in a tube closed at both ends by plates. In testing this method, I had occasion to make some observations which have probably also been made by others, but which I have nowhere found mentioned.

Influence of the side of the Tube.

If the rays from any source of heat be allowed to act upon a thermo-pile without having passed through any tube, a smaller deflection is obtained than when the rays from the same source of heat placed at the same distance from the thermo-pile are allowed to pass through a tube open at both ends, that is, not closed by any kind of plate. This increased action is obviously caused by the rays reflected from the inner side of the tube, so that the thermo-pile is acted on, not only by the rays which come directly from the source of heat, but also by those which fall obliquely into the tube and are again reflected. Even if the tube were blackened on the inside, or if, as was usually the case in the following

experiments, the inside was lined with a black, rough, non-lustrous paper, the action was likewise stronger than without a tube, although the increase was not so considerable as if the glass was without this coating.

The influence of the tube can, it is true, be diminished by introducing diaphragms, which hinder the irradiation of the inside; but I have not succeeded in entirely obviating it, for the edges of the diaphragm likewise reflect heat. But the significance of the action of the sides of the tube in investigating the diathermancy of gases is best seen from the following experiments.

In these experiments a strong gas-flame with a double draught, surrounded by a glass cylinder, was used as a source of heat. It was provided with a small parabolic metallic mirror, which reflected the rays of the lamp in such a manner that they passed into a tube 1 metre long and 35 millims. in internal diameter, at the other end of which was the above-named thermo-electric pile. Between the tube and the lamp, and somewhat nearer the latter, there was a screen consisting of two metal plates at a distance of 12 millims. from each other. This could be removed when the rays were to fall on the pile, and replaced as soon as this was finished. Between this screen and the tube was a second similar screen, which had an aperture 30 millims. square, the centre of which was in a line with the axis of the tube. This screen, which was always in a fixed position, protected the rays of the lamp from the outside of the tube when the other was removed. There was another screen with a similar aperture close to the thermo-pile and between it and the tube, the object of which was to protect the pile from all external rays.

When using this arrangement, the rays were allowed to pass through a tube open at both ends, and lined with rough black paper; the deflection of the needle amounted to $24^{\circ}{\cdot}7$, corresponding to 32·2 units. On removing the tube the deflection was only 10°, corresponding to 10. If, after removing the tube, the rays were allowed to pass through two glass plates 4 millims. thick, placed at the same distance at which they would be if they closed the tube, the deflection of the needle would only be 1° to 2°. If, on the contrary, the blackened tube, as the tube lined with black paper will for the future be called, was between the glass plates, the deflection increased to $12^{\circ}{\cdot}6$, corresponding to 12·6. If the tube closed with glass plates was not blackened on the inside, the deflection increased to 64°, corresponding to 320.

From this it will be seen how greatly investigations on the passage of heat-rays will be affected by the nature of the tubes in which the gases are experimented on.

It is known that light presents a deportment entirely similar to radiant heat. If passed through a tube, it produces a far greater illumination than without the use of a tube. This phenomenon, which depends on repeated reflexion from the inner side of the tube, is especially evident in the case of the so-called liquid jet. Even if the tube is lined on the inside with black paper, it produces, as I have found, a very surprising increase in the illumination, although not so intense as the unblackened tube.

In order the better to ascertain the influence of the side of the tube in the passage of heat-rays, I have investigated, in the case of each gas, the radiation, not only of the tube blackened internally, or rather lined with black paper, but also with the unblackened tube.

In the following Table the results obtained are placed opposite each other for the sake of easier comparison.

As the gas-flame could not always be obtained of exactly the same intensity, the radiation of each individual gas was compared with that of atmospheric air, so that both were investigated directly after each other. The relation between the two is given in the Table for each gas.

In all the experiments the tube was the same, as also were the glass plates by which it was closed. The determinations were made in the manner described on page 88; the mean was taken of the position of equilibrium of the needle before and after each deflection, and this was subtracted from the observed deflection. The mean of four such determinations for atmospheric air, and of the same number for each gas, gives the relation of the radiation between the two.

The gases were prepared in the manner already described.

Transmission of the Heat of a Gas-flame.

Blackened tube.					Unblackened tube.				
No.	Position of equilibrium of the needle.	Mean.	Observed deflection.	Difference.	No.	Position of equilibrium of the needle.	Mean.	Observed deflection.	Difference.
Atmospheric air under 1 atm. pressure.									
I	+0°·5	+0°·6	13°·2	12°·6		+0°·5	+0°·6	62°·5	61°·9
	+0·7	0·7	13·5	12·8		+0·7	0·7	63·0	62·3
	0·7	0·7	13·2	12·5		0·7	0·7	62·7	62·0
	0·7	0·7	13·5	12·8		0·5	0·7	63·0	62·4
	0·7					0·6			
	Mean			12·67		Mean			62·15
	Corresponding to ...			12·67		Corresponding to ...			273

Transmission of the Heat of a Gas-flame (*continued*).

Blackened tube.					Unblackened tube.				
No.	Position of equilibrium of the needle.	Mean.	Observed deflection.	Difference.	No.	Position of equilibrium of the needle.	Mean.	Observed deflection.	Difference.
	Atmospheric air under 4 millims. pressure.								
I*a*	+0°·5 0·0 +0·5 0·5 0·5	+0°·25 0·25 0·50 0·50	13°·5 13·2 13·5 13·5	13°·25 13·0 13·0 13·0	I*a*	+0°·5 0·5 0·5 0·5 0·5	+0°·5 0·5 0·5 0·5	64°·5 64·5 64·5 64·5	64°·0 64·0 64·0 64·0
	Mean..... 13·0 Corresponding to ..... 13·0 I : I*a* = 12·67 : 13·0 = 100 : 102·60					Mean..... 64·0 Corresponding to.....320·0 I : I*a* = 273 : 320 = 100 : 117·21			
	Atmospheric air under 1 atm. pressure.								
II	−0·5 0·0 −0·2 −1·0 −1·0	−0·25 −0·1 −0·6 −1·0	12·5 12·7 12·2 12·0	12·75 12·8 12·8 13·0	II	+0·5 +0·5 +0·7 +1·0 +1·0	+0·5 0·6 0·85 1·0	61·0 61·0 61·2 61·5	60·5 60·4 60·35 60·5
	Mean..... 12·8 Corresponding to..... 12·8					Mean ..... 60·45 Corresponding to ... 236·9			
	Atmospheric air under 6 millims. pres.					Atmospheric air under 8 millims. pres.			
II*a*	0·0 −0·25 −0·25 −0·5 −0·5	−0·1 −0·25 −0·3 −0·5	13·0 12·75 12·75 12·5	13·1 13·0 13·1 13·0	II*a*	+0·2 0·5 0·5 0·5 0·5	+0·35 0·5 0·5 0·5	62·7 62·7 62·7 63·0	62·35 62·2 62·2 62·5
	Mean ..... 13·1 Corresponding to ... 13·1 II : II*a* = 12·8 : 13·1 = 100 : 102·3					Mean ..... 62·3 Corresponding to ... 277·0 II : II*a* = 236 : 277 = 100 : 117·37			
	Atmospheric air under 1 atm. pressure.								
III*	0·0 0·0 +0·5 +0·5 +0·5	0·0 +0·25 0·5 0·5	11·0 11·2 11·5 11·5	11·0 11·0 11·0 11·0	III	+2·0 1·0 1·0 1·5 1·5	+1·5 1·0 1·25 1·5	59·5 59·0 59·25 59·5	58·0 58·0 58·0 58·0
	Mean ..... 11·0 Corresponding to..... 11·0					Mean ..... 58·0 Corresponding to ... 193·0			

* In this determination a greater resistance was introduced in the connecting wire of the thermo-pile.

Transmission of the Heat of a Gas-flame (*continued*).

Blackened tube.					Unblackened tube.				
No.	Position of equilibrium of the needle.	Mean.	Observed deflection.	Difference.	No.	Position of equilibrium of the needle.	Mean.	Observed deflection.	Difference.
	Oxygen under 1 atm. pressure.								
III*a*	+0°·5 +0·2 0·0 0·0 +0·2	+0°·3 +0·1 0·0 +0·1	11°·5 11·0 11·0 11·0	11°·2 10·9 11·0 10·9	III*a*	+1°· 1·25 0·75 1·0 0·0	+1°·1 1·0 0·8 0·5	59°·0 59·25 58·75 58·5	57°·9 58·25 57·9 58
	Mean ..... 11·0 Corresponding to ... 11·0 III : III*a* = 11·0 : 11·0 = 100 : 100					Mean ..... 58·0 Corresponding to ... 193·0 III : III*a* = 193 : 193 = 100 : 100			
	Atmospheric air under 1 atm. pressure.								
IV	+0·5 +0·5 0·7 0·7 0·7	+0·5 0·6 0·7 0·7	16·7 16·7 16·7 17·0	16·2 16·1 16·0 16·3	IV	+0·5 +1·0 1·0 1·0 1·0	+0·75 1·0 1·0 1·0	61·0 61·2 61·2 61·2	60·25 60·2 60·2 60·2
	Mean ..... 16·15 Corresponding to ... 17·40					Mean ..... 60·2 Corresponding to ... 230·0			
	Hydrogen under 1 atm. pressure.								
IV*a*	+0·7 0·5 0·5 0·5 0·5	+0·6 0·5 0·5 0·5	16·7 16·5 16·7 16·5	16·1 16·0 16·2 16·0	IV*a*	+1·0 0·7 1·0 0·7 0·7	+0·85 +0·85 +0·85 0·70	61·0 61·0 60·7 60·5	60·15 60·15 59·85 59·8
	Mean ..... 16·07 Corresponding to ... 17·2 IV : IV*a* = 17·4 : 17·2 = 100 : 98·85					Mean ..... 60·0 Corresponding to ... 226·0 IV : IV*a* = 230 : 226 = 100 : 98·26			
	Atmospheric air under 1 atm. pressure.								
V	−1·2 −1·0 0·7 0·5 0·5	−1·1 −0·8 −0·6 −0·5	17·7 18·0 18·2 18·5	18·8 18·8 18·8 19·0	V	+0·5 +0·7 0·5 0·5 0·5	+0·6 +0·6 0·5 0·5	61·0 61·0 61·2 61·2	60·4 60·4 60·7 60·7
	Mean ..... 18·8 Corresponding to ... 22·1					Mean ..... 60·55 Corresponding to ... 238·0			

Transmission of the Heat of a Gas-flame (*continued*).

Blackened tube.					Unblackened tube.				
No.	Position of equilibrium of the needle.	Mean.	Observed deflection.	Difference.	No.	Position of equilibrium of the needle.	Mean.	Observed deflection.	Difference.
Carbonic acid under 1 atm. pressure.									
V*a*	−0°·5	−0°·35	17°·7	18°·0	V*a*	+0°·2	+0°·35	60°·0	59°·65
	−0·2	−0·35	17·7	18·0		0·5	0·5	60·2	59·7
	−0·5	−0·25	17·7	17·95		0·5	0·6	60·2	59·6
	0·0	0·0	18·0	18·0		0·7	0·7	60·2	59·5
	0·0					0·7			
Mean ..... 18·0					Mean ..... 59·6				
Corresponding to ... 20·8					Corresponding to ... 218·0				
V : V*a* = 22·1 : 20·8 = 100 : 94·11					V : V*a* = 238 : 218 = 100 : 91·59				
Atmospheric air under 1 atm. pressure.									
VI	0	0·0	23·75	23·75	VI	+0·7	+1·0	63·0	62·0
	0	+0·1	24·0	23·9		+1·2	1·2	63·0	61·8
	+0·2	0·1	24·2	24·1		1·2	1·2	63·0	61·8
	0	0·0	24·0	24·0		1·2	1·2	63·0	61·8
	0					1·2			
Mean..... 23·9					Mean ..... 61·8				
Corresponding to..... 30·8					Corresponding to ... 265·0				
Carbonic oxide under 1 atm. pressure.									
VI*a*	+0·2	+0·2	23·0	22·8	VI*a*	0·0	+0·25	60·25	60·0
	0·2	0·2	23·0	22·8		+0·5	+0·50	60·3	59·8
	0·2	0·1	23·0	22·9		0·5	0·75	60·75	60·0
	0·0	0·0	22·5	22·5		1·0	0·85	60·75	59·9
	0·0					0·7			
Mean ..... 22·75					Mean ..... 59·9				
Corresponding to ... 29·00					Corresponding to ... 224·0				
VI : VI*a* 30·8 : 29·0 = 100 : 94·15					VI : VI*a* = 265 : 224 = 100 : 84·52				
Atmospheric air under 1 atm. pressure.									
VII	+0·7	+0·7	16·7	16·0	VII	−0·5	−0·35	60·5	60·85
	0·7	0·6	16·7	16·1		−0·2	−0·2	60·7	60·9
	0·5	0·5	16·5	16·0		−0·2	−0·2	60·7	60·9
	0·5	0·5	16·5	16·0		−0·2	−0·2	60·7	60·9
	0·5					−0·2			
Mean ..... 16·0					Mean ..... 60·9				
Corresponding to ... 17·1					Corresponding to ... 245·0				

Transmission of the Heat of a Gas-flame (*continued*).

Blackened tube.					Unblackened tube.				
No.	Position of equilibrium of the needle.	Mean.	Observed deflection.	Difference.	No.	Position of equilibrium of the needle.	Mean.	Observed deflection.	Difference.
	Protoxide of nitrogen under 1 atm. pressure.								
VII*a*	+0°5	+0°35	15°2	14°85	VII*a*	0°0	+0°25	60°0	59°75
	0·2	0·1	15·2	15·1		+0·5	+0·6	60·2	59·6
	0·0	0·0	15·2	15·2		+0·7	+0·7	60·5	58·8
	0·0	0 0	15·2	15·2		0·7	+0·7	60·7	60·0
	0·0					0·7			
	Mean			15·1		Mean			59·5
	Corresponding to ...			15·4		Corresponding to ...			217·0
VII : VII*a* = 17·1 : 15·4 = 100 : 90·05					VII : VII*a* = 245 : 217 = 100 : 88·57				
	Atmospheric air under 1 atm. pressure.								
VIII	0	0	22·5	22·5	VIII	+0·75	0·85	65·2	64·35
	0	0	22·7	22·7		+1·0	1·0	65·2	64·2
	0	+0·1	23·0	22·8		1·0	1·1	65·2	64·1
	+0·2	+0·35	23·0	22·65		1·2	1·2	65·5	64·3
	+0·5					1·2			
	Mean			22·7		Mean			64·2
	Corresponding to ...			29·0		Corresponding to ...			326 0
	Marsh-gas under 1 atm. pressure.								
VIII*a*	0	0	22·5	22·5	VIII*a*	+1·0	+1·25	64·0	62·75
	0	+0·1	22·5	22·4		1·5	1·5	64·5	63·0
	+0·2	0·35	22·7	22·35		1·5	1·6	64·5	62·9
	0 5	0·5	22·7	22·2		1·7	1·6	64·7	63·1
	0·5					1·5			
	Mean			22·35		Mean			62·95
	Corresponding to ...			28 5		Corresponding to ...			293·0
VIII : VIII*a* = 29·0 : 28·5 = 100 : 98·27					VIII : VIII*a* = 326 : 293 = 100 : 89·87				
	Atmospheric air under 1 atm. pressure.								
IX	0	0	18·0	18·0	IX	+0·5	+0·6	62·5	61·9
	0	0	18·2	18·2		0·7	0·7	63 0	62·3
	0	0	18·2	18·2		0·7	0·7	62·7	62·0
	0	0	18·2	18·2		0·5	0·7	63·0	62·4
	0					0 7	0 6		
	Mean			18·15		Mean			62 15
	Corresponding to ...			21·0		Corresponding to ...			273·0

Transmission of the Heat of a Gas-flame (*continued*).

Blackened tube.					Unblackened tube.				
No.	Position of equilibrium of the needle.	Mean.	Observed deflection.	Difference.	No.	Position of equilibrium of the needle.	Mean.	Observed deflection.	Difference.
			Olefiant gas under 1 atm. pressure.						
IX*a*	0° −0·2 −0·2 0 0	−0°·1 −0·2 −0·1 0	14°·0 13·7 13·5 13·7	14°·1 13·9 13·6 13·7	IX*a*	+0°·5 0·5 0·5 0·5 0·5	+0°·5 0·5 0·5 0·5	58°·5 58·5 58·2 58·2	58°·0 58·0 57·7 57·7
	Mean ..... 13·8 Corresponding to ... 13·8 IX : IX*a* = 21·0 : 13·8 = 100 : 65·71					Mean ..... 57·85 Corresponding to ... 192·0 IX : IX*a* = 273 : 192 = 100 : 70·33			
			Atmospheric air under 1 atm. pressure.						
X	+1·0 +1·2 0 0 0	+1·1 +0·6 0 0	27·5 27·0 26·2 26·5	26·4 26·4 26·2 26·5	X	+0·75 0·5 0·0 0·5 0·5	+0·6 0·25 0·25 0·5	65·0 65·0 64·7 64·7	64·4 64·75 64·45 64·2
	Mean ..... 26·4 Corresponding to ... 35·8					Mean ..... 64·45 Corresponding to ... 334·0			
			Olefiant gas under 1 atm. pressure.						
X*a*	+0·25 0 +0·5 0·5 0·5	+0·12 0·25 0·5 0·5	20·0 20·5 20·5 20·25	19·9 20·25 20·0 19·75	X*a*	−1·0 −1·2 0 0 0	−1·1 −0·6 0 0	60·0 60·2 60·75 60·75	61·1 60·8 60·75 60·75
	Mean ..... 20·0 Corresponding to ... 24·0 X : X*a* = 35·8 : 24·0 = 100 : 67·03					Mean ..... 60·6 Corresponding to ... 239·0 X : X*a* = 334 : 239 = 100 : 71·55			
			Atmospheric air under 1 atm. pressure.						
XI	+0·7 +1·0 +0·5 0·5 0·5	+0·85 0·75 0·50 0·5	18·0 17·7 17·7 17·5	17·15 17·0 17·2 17·0	XI.	0 +0·5 0·5 0·5 0·5	+0·25 0·5 0·5 0·5	62·0 62·5 62·2 62·2	61·75 62·0 61·7 61·7
	Mean ..... 17·1 Corresponding to ... 19·3					Mean ..... 61·8 Corresponding to ... 265·0			

Transmission of the Heat of a Gas-flame (*continued*).

Blackened tube.					Unblackened tube.				
No.	Position of equilibrium of the needle.	Mean.	Observed deflection.	Difference.	No.	Position of equilibrium of the needle.	Mean.	Observed deflection.	Difference.
			Ammonia under 1 atm. pressure.						
XI*a*	0° +0·5 0·7 0·7 0·7	+0°·25 0·6 0·7 0·7	11°·7 12·0 12·2 12·2	11°·45 11·4 11·5 11·5	XI*a*	+0°·5 0·7 0·7 0·7 0·7	+0°·6 0·7 0·7	56°·5 56·7 56·5 56·7	55°·9 56·0 55·8 56·0
	Mean			11·5		Mean			55·9
	Corresponding to ...			11·5		Corresponding to ...			224·0
	XI : XI*a* = 19·3 : 11·5 = 100 : 59·58					XI : XI*a* = 265 : 224 = 100 : 84·52			

The relation of the radiation through the various gases is therefore the following:—

	Blackened tube.	Unblackened tube.
Atmospheric air under 1 atm. . .	100	100
„ „ 4 millims. .	102·6	117·21
„ „ 6 millims. .	102·3	
„ „ 8 millims. .		117·37
Oxygen under 1 atm. . .	100	100
Hydrogen „ „	98·85	98·26
Carbonic acid „ „	94·11	91·59
Carbonic oxide „ „	94·15	84·52
Protoxide of nitrogen „	90·05	88·57
Marsh-gas „ „	98·27	89·87
Olefiant gas „ „	65·71 67·03	70·33 71·55
Ammonia* „ „	59·58	84·52

Since oxygen gave the same values as atmospheric air, it appeared superfluous to investigate nitrogen.

The great difference between the radiation in rarefied space, according as it is investigated in the blackened or in the unblackened tube, led me to determine it once more. This was done with the unblackened tube in such a manner that the deflections were smaller than before, which was effected by remo-

* As the tube had to be filled with ammonia without using the air-pump, it is not improbable that small quantities of atmospheric air may have been left; for in passing the gas through a tube 35 millims. wide, it is very difficult to expel the air completely. For this reason I have not examined cyanogen.

ving the mirror with which the lamp was provided, and placing the lamp itself at greater distances. For it appeared not improbable that the values of the galvanometer corresponding to the deflections, which for greater deflections are not capable of such exact determination as for smaller ones, might have caused the great difference in the radiation through rarefied air, and through air at the normal pressure. With the blackened tube it was not possible to dispense with the mirror; and hence, in order to produce a smaller deflection, the resistance of the wire connected with the galvanometer was increased. In this way the following values were obtained:—

Blackened tube.					Unblackened tube.				
No.	Position of equilibrium of the needle.	Mean.	Observed deflection.	Difference.	No.	Position of equilibrium of the needle.	Mean.	Observed deflection.	Difference.
Atmospheric air under 1 atm. pressure.									
I	0·0 0 0 +0·5 +0·5 +0·5	0·0 +0·25 +0 5 +0·5	11·0 11·25 11·5 11·5	11·0 11·0 11·0 11·0	I	+0·75 +2·0 +2·0 +1·75 +2·0	+1·75 +2·0 +1·75 +1·75	28·25 28·75 28·50 28·50	26·50 26·75 26·75 26·75
	Mean ..... 11·0 Corresponding to ... 11·0					Mean ..... 26·70 Corresponding to ... 36·3			
Under 4 millims. pressure.					Under 9 millims. pressure.				
I*a*	+0·5 +0·5 +0·5 +0·5 +0·5	+0·5 +0·5 +0·5 +0·5	12·0 11·75 12·0 12·0	11·5 11·25 11·5 11·5	I*a*	0·0 0·0 −0·2 −0·2 −0·2	0·0 −0·1 −0·2 −0·2	29·0 29·0 29·0 29·0	29·0 29·1 29·2 29·2
	Mean ..... 11·5 Corresponding to ... 11·5 I : I*a* = 11·0 : 11·5 = 100 : 103·8					Mean ..... 29·1 Corresponding to ... 41·2 I : I*a* = 36·3 : 41·2 = 100 : 113·5			
Unblackened tube.									
Atmospheric air under 1 atm. pres.					Atmospheric air under 6 millims. pres.				
II	−1·0 −1·0 −1·0 −1·0 −1·0	−1·0 −1·0 −1·0 −1·0	13·0 13·0 13·0 13·0	14·0 14·0 14·0 14·0	II*a*	−1·5 −1·5 −1·5 −1·5 −1·5	−1·5 −1·5 −1·5 −1·5	14·7 14·7 14·5 15 0	16·2 16·2 16·0 16·5
	Mean ..... 14·0 Corresponding to ... 14·0					Mean ..... 16·2 Corresponding to ... 16·4 II : II*a* = 14 : 16·4 = 100 : 117·1			

Table (*continued*).

Unblackened tube.									
No.	Position of equilibrium of the needle.	Mean.	Observed deflection.	Difference.	No.	Position of equilibrium of the needle.	Mean.	Observed deflection.	Difference.
Atmospheric air under 1 atm. pressure.					Under 6 millims. pressure.				
III	0·0				IIIa	0·0			
	+0·2	+0·1	11·7	11·6		0·0	0·0	12·7	12·7
	+0·2	+0·2	11·5	11·3		0·0	0·0	13·0	13·0
	+0·0	+0·1	11·5	11·4		0 0	0·0	13·0	13·0
	+0·2	+0·1	11·5	11·4		0 0	0·0	13·2	13·2
	Mean			11·4		Mean			13·2
	Corresponding to ...			11·4		Corresponding to ...			13·2
					III : III a = 11·4 : 13·2 = 100 : 113·2				

As in these determinations differences were obtained similar to those already mentioned, in calculating the relation between the radiation through vacuum and through different gases, I have taken as a basis the values previously found and detailed in page 103. In so doing I have assumed that, if 100 rays pass through atmospheric air under 1 atm. pressure, by using the blackened tube 102·5, and the unblackened tube 117·3, would pass through vacuum. Hence of 100 rays from a gas-flame which pass through vacuum, the following quantities pass through the various gases under the pressure of one atmosphere:—

	Blackened tube.	Unblackened tube.
Vacuum	100	100
Atmospheric air . .	97·56	85·25
Oxygen	97·56	85·25
Hydrogen	96·43	83·77
Carbonic acid . . .	91·81	78·08
Carbonic oxide . . .	91·85	72·05
Protoxide of nitrogen .	87·85	75·50
Marsh-gas	95·87	76·61
Olefiant gas . .	64·10 65·39	59·96 60·99
Ammonia	58·12	55·00

Influence of Aqueous Vapour on Radiation.

Although it might with certainty be predicted that the small quantity of aqueous vapour which air can take up at the ordinary

temperature (not 2 per cent. of its volume at 16° C.) could exercise no influence on the radiation, it appeared desirable to determine experimentally that this supposition was correct. With this view I made comparative determinations of the radiation through perfectly dry air and through air entirely saturated with moisture. The air was passed through several chloride of calcium tubes, and afterwards, by means of a respirator, was drawn through the unblackened tube in such quantity that all the air previously in the tube might be considered as displaced. After the radiation was determined, the air was exhausted by means of the air-pump, and fresh air admitted, which before entering had passed through water slowly and in small bubbles. This air was then exhausted, and a fresh quantity admitted under the same circumstances. After moist air had been thrice successively admitted, it could be assumed that the whole of the air contained in the tube at the temperature 16° C., and pressure 764·6 millims., was saturated with aqueous vapour.

The capacity of dry air and moist air to transmit heat-rays of 100° C. was investigated in exactly the same way by means of the apparatus described in page 87. In this way the following results were obtained:—

Dry air.				Air saturated with aqueous vapour.			
Position of equilibrium of the needle.	Mean.	Observed deflection.	Difference.	Position of equilibrium of the needle.	Mean.	Observed deflection.	Difference.
			With the Gas-lamp.				
0·0	−0·25	11·5	11·75	−1·5	−1·35	10·75	12·1
−0·5	−0·25	11·5	11·75	−1·25	−1·25	10·5	11·75
0·0	0·0	12·0	12·0	−1·25	−1·25	10·75	12·0
0·0	0·0	12·0	12·0	−1·25	−1·25	10·5	11·75
0·0				−1·25			
	Mean.....		12·0		Mean		12·0
			With the source of heat at 100° C.				
0·0	.....	12·5	.....	+0·5	+0·5	13·25	12·75
0·0	.....	12·75	.....	+0·5	+0·5	13·25	12·75
0·0	.....	12·5	.....	+0·5	+0·5	13·0	12·5
0·0	.....	12·5	.....	+0·5	+0·75	13·5	12·75
				+1·0			
	Mean		12·5		Mean		12·6

These experiments show that the water present in the atmosphere at 16° C. exercises no perceptible influence on the radiation. That such an influence should be felt as soon as part of the vapour separates as fog, appears very probable.

XV. *On New Falls of Meteoric Stones.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

SOME weeks ago I received a letter from Professor Joaquin Balcells of Barcelona, stating that he had heard of a large fall of meteorites, accompanied by tremendous detonations, said to have taken place at Cañellas, near Villanova in Catalonia, at some distance from Barcelona, on the 14th of May this year. I have just received from him another letter dated the 27th of June, enclosing an account of his expedition to Cañellas for the purpose of procuring additional information, and also, if possible, some specimens. I give the following translation of this part of his letter:—

"There is no doubt that stones really fell on May 14 at about 1 P.M.; but the greater number are lost, from having fallen with such violence upon the arable land that they could not be found. Two or three fell, however, upon rocks, which they penetrated and cut up to a depth of 5 inches (*pouces*) in a direction towards N.E. at an angle of 45°. They broke into pieces with a tremendous noise and great light. The largest specimen only weighed 18 ounces, and is already destined for the Natural History Museum at Madrid. The second specimen which I saw, was destined for the Professor of Physics, Señor Arbá of Barcelona. I likewise saw other specimens of from 5 to 9 grammes in weight, which were in the hands of the peasants, who would not part with them at any price, because they fancied that these stones, coming from heaven, would bring them good luck. From this cause I was only able to procure for myself one small fragment of 5 grammes weight."

An aërolitic fall is mentioned in 'Cosmos' for April 26, 1861, as having taken place at Tocane St. Apre in Dordogne, France; an aërolite fell on the 14th of February, 1861, with a streak of fire (without noise apparently), in the market place of that town; it weighed only 7 grammes, and is now deposited in the museum of the department at Dordogne.

Another meteoric stone in all probability fell last year on the 8th or 9th of June, about two miles from Raphoe in Co. Donegal, Ireland, on the farm of Dr. M'Clintock of Raphoe, about 2 P.M. It was about the size of a hen's egg, and fell during a storm of thunder, lightning, and hail. It resembled a friable sandstone; but it does not appear there was either any black crust to it, or that there was any fire-ball seen at the time. This fall is mentioned in the 'Londonderry Sentinel' of June 15, 1860. It appears that one portion of this stone has been lost or mislaid,

and the remainder had crumbled into sand and has been thrown away. When it fell it broke into three pieces, and was cold and saturated with wet; it was seen to fall by a ploughman of Dr. M'Clintock's, and immediately afterwards picked up by him.

R. P. Greg.

Manchester, July 12, 1861.

XVI. *Solution of a Problem in the Calculus of Variations.* *By* Professor Challis*.

In the July Number of this Magazine the Astronomer Royal has called attention to a problem in the calculus of variations, the solution of which presents some difficulty. The method of solution I am about to propose, which appears to meet the difficulty, is, I believe, new.

The following is the enunciation of the problem as given in Mr. Todhunter's 'History of the Calculus of Variations':—To construct upon a given base AB a curve, such that the superficial area of the surface generated by its revolution round AB may be given, and that its solid content may be a maximum. By the rules of the calculus of variations, the ordinary notation being adopted, the solution of the problem is given by the equation

$$\delta . \int (y^2 + 2ay\sqrt{1+p^2})dx = 0.$$

Integrating from $x=0$ to $x=x_1$, and equating separately to zero the parts outside and those under the sign of integration, we have

$$\frac{ap_1 y_1 \delta y_1}{\sqrt{1+p_1^2}} - \frac{ap_0 y_0 \delta y_0}{\sqrt{1+p_0^2}} = 0,$$

$$y + a\sqrt{1+p^2} - \frac{d}{dx}\frac{ayp}{\sqrt{1+p^2}} = 0.$$

The first equation is evidently satisfied by the hypothesis that $y_0 \delta y_0 = 0$ and $y_1 \delta y_1 = 0$. The integration of the other gives

$$\frac{2ay}{\sqrt{1+p^2}} = b - y^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

b being the arbitrary constant introduced by the integration.

The next step usually taken in solving this problem is to put $b=0$, because at the fixed points A and B y vanishes. This appears to have been done previous to the second integration solely because the equation (1) is not integrable unless $b=0$. I

* Communicated by the Author.

shall now show that the effect of this step is to restrict the generality of the solution.

Although the above differential equation is not generally integrable, we can obtain from it an exact expression for the length s of the curve. For we have

$$\frac{ds}{dx} = p\frac{ds}{dy} = \frac{2ay}{b-y^2},$$

and

$$p^2 = \left(\frac{2ay}{b-y^2}\right)^2 - 1;$$

$$\therefore\ ds = \frac{2ay\,dy}{\sqrt{4a^2y^2-(b-y^2)^2}}.$$

The integration of this equation gives

$$\cos\left(\frac{s}{a}+k\right) = \frac{2a^2+b-y^2}{2a\sqrt{a^2+b}}, \quad . \quad . \quad . \quad . \quad (2)$$

k being a new arbitrary constant. Now at the point A, $y=0$ and $s=0$. Hence

$$\cos k = \frac{2a^2+b}{2a\sqrt{a^2+b}} = \frac{2a^2+b}{\sqrt{(2a^2+b)^2-b^2}}. \quad . \quad . \quad (3)$$

Thus the denominator of this value of $\cos k$ is less than the numerator; which is impossible. If s_1 be the length of the arc, it would similarly be found, for the point B, that $\cos\left(\frac{s_1}{a}+k\right)$ is greater than unity. The inference to be drawn from these results is, that when $s=0$ and $s=s_1$, y cannot be equal to zero, but must have some other values, which it is required to find before the solution of the question can proceed. This may be done as follows.

Since the coordinates of the extremities of the arc must have certain values y_0 and y_1 different from zero, it is *necessary* that the circular areas generated by the revolution of these coordinates should be taken into account in the expression for the total surface of the solid; that is, the reasoning must be conducted in the manner proposed by Mr. Todhunter (p. 410). Hence, if r and r' be distances of points of the circular areas from A and B respectively, there will be the two additional terms,

$$\delta\int ar\,dr + \delta\int ar'\,dr',$$

the integrations being taken from $r=0$ to $r=y_0$, and from $r'=0$ to $r'=y_1$. Consequently the total quantity outside the sign of integration will be

$$\left(\frac{p_1}{\sqrt{1+p_1^2}}+1\right)ay_1\,\delta y_1 - \left(\frac{ap_0}{\sqrt{1+p_0^2}}-1\right)a\,y_0\,\delta y_0.$$

Now, since y_0 and y_1 are quantities to be determined, neither δy_0 nor δy_1 is equal to zero. Hence

$$\frac{p_1}{\sqrt{1+p_1^2}}+1=0 \text{ and } \frac{p_0}{\sqrt{1+p_0^2}}-1=0;$$

that is, $p_0=+\infty$ and $p_1=-\infty$. Hence if C and D be the extremities of the ordinates, the curve is at these points continuous with the ordinates; also the equation (1) shows, by putting $p=\infty$, that for each of the points $y^2=b$.

The equation (2) between y and s being put under the form

$$y^2=2a^2+b-2a\sqrt{a^2+b}\cos\left(\frac{s}{a}+k\right), \quad . \quad . \quad . \quad (4)$$

it will be seen that in the case in which $b=0$, and consequently by (3) $\cos k=1$ and $k=0$, we shall have

$$y^2=2a^2\left(1-\cos\frac{s}{a}\right),$$

$$\text{or } y=2a\sin\frac{s}{2a}.$$

The curve is therefore a semicircle, the radius of which is $2a$. Thus it appears that this is a particular instance contained in the general solution.

The equation (4) shows that for any other value of b the curve differs from a semicircle. Its length from C to D is readily found. For since $y_0=y_1$, it follows from (3) that

$$\cos k=\cos\left(\frac{s_1}{a}+k\right).$$

Hence $\frac{s_1}{a}=2\pi$, or $s_1=2\pi a=$ half the circumference of a circle whose radius is $2a$.

Also the area enclosed by the curve, the ordinates AC and BD, and the base AB, can be exactly determined. For from the equation

$$p^2=\left(\frac{2ay}{b-y^2}\right)^2-1$$

we obtain

$$ydx=-\frac{(b-y^2)y\,dy}{\sqrt{4a^2y^2-(b-y^2)^2}},$$

which being integrated from A to B, gives for the above-mentioned area

$$2a\sqrt{b}+2\pi a^2.$$

If the equation (1) were integrable, the values of the three arbitrary constants might be found from the given value of the en-

closing surface, and the given condition that the surface passes through A and B.

The solid resulting from the foregoing investigation possesses the characteristic of a maximum, and is the only solution which the problem admits of. It is antecedently evident that the conditions of the question must admit of being satisfied by a *surface* of some kind passing through the given points, and that consequently the calculus of variations could not fail to give such a solution.

Cambridge Observatory,
July 19, 1861.

XVII. *On the Action of certain Vapours on Films; on the Motions of Creosote on the surface of Water, and other phenomena. In a Letter addressed to* W. A. Miller, *Esq., M.D., F.R.S. &c., Professor of Chemistry, King's College, London. By* Charles Tomlinson, *Lecturer on Science, King's College School*.*

My dear Miller,

A FEW days ago, after a lecture at College on *Cohesion and Adhesion*, one of my pupils asked me, "What is the cause of the remarkable agitation that takes place when sulphuric ether is dropped on the surface of water?" I put that same question to myself more than five and twenty years ago while studying chemistry, and made a large number of experiments on the subject, some of which I have lately had the pleasure of showing you. As you were kind enough to express great interest in them, and a desire that I would complete the inquiry by pushing it to a definite conclusion, I have endeavoured to do so, and will with your permission submit the whole inquiry to you from my own point of view.

But in order to do this I must go back to the years 1837–38, when I obtained a large number of results, and embodied them in three Articles which are now before me in MS. I did not publish them, because the conclusions were not quite satisfactory to my own mind. But being engaged about that time in seeing my 'Students' Manual of Natural Philosophy' through the press, I included the principal experimental results in that work, where you will find them at pages 545–549, and again at pages 553–555. The popular nature of this work doubtless caused these experiments to remain unknown to scientific men; and I venture to think that they will even now strike many with an air of novelty. This is my excuse for a short summary of old results by way of introduction to new ones, or, at least, to such as have not been published.

* Communicated by Dr. Miller.

One of my first experiments, made with the view to ascertain what takes place when ether is brought into contact with water, was the following:—A goblet being quite filled with water was placed in a good light, and the finger well wetted with ether was brought down very near the surface. On looking along this surface in the direction of the light, a cup-shaped depression was evident. I then dusted the surface of the water with a light powder, such as lycopodium, and on presenting the finger wet with ether there was a strong repulsive action; the powder was forcibly driven aside, and the surface of the water was laid bare, evidently in a state of agitation under the influence of the vapour of ether. I tried many sorts of powders with a similar result, but none answered better than lycopodium.

My next proceeding was to try to represent the action of the ether by means of films of oily compounds formed by the spreading of an oily drop on the surface of water. A large soda-water glass was first employed, but a common white dinner-plate showed the effects best. Oil of turpentine, many of the turpentine varnishes, such as gold-size, black Japan, carriage, copal, &c., make admirable films. Some of the fixed and essential oils also answer very well. In experiments of this kind, a single drop of the oily substance must be gently placed on, or rather *delivered* to the water without any fall or disturbance; otherwise the varnish, &c. may sink below the surface in the form of a perfectly spherical bead, and so remain as a good example of cohesion. The best method of obtaining a film is to dip a glass rod into one of the oily liquids, and allow it to drain so that it may deliver only a single drop to the surface of the water. The plate filled with water should be placed before a good light, when a drop of the oily substance, being gently placed on the centre, usually spreads out with a beautiful exhibition of colour, or the film may be quite colourless. Take the latter case. The finger, or, what is better, a flat piece of sponge tied with thread over the rounded end of a glass stirring-rod, wetted with ether and held over the film, produces a cup-shaped cavity, within which a beautiful set of Newton's rings may be seen so long as the sponge is wet with ether. In this case the vapour of ether attenuates the film; the point immediately below the sponge is the point of greatest action, and here the black of the centre of the first series of rings is seen: the action diminishes from this depressed point, where the film is thinnest, and it gradually increases in thickness until it unites with the rest of the film, where colour ceases to be displayed. The film is in fact under tension so long as the ether vapour is acting upon it; and the tension is greatest in the direction of a vertical ray from the sponge to the water, and gradually diminishes as the rays increase in length from the sponge to the

water. Now as these thicknesses vary, for water, from about the 0·38 to the 57·75 millionths of an inch, the film gives all or some of the series of colours known as Newton's rings. In the second place, supposing the drop of oil, &c. to form a coloured film (and most of the turpentine varnishes do so to perfection), the ether-sponge still developes a system of rings, not always beginning with those of the first series, but exhibiting the colours of the second, third, fourth, &c. The diameter of the coloured rings on the oil-film may vary from $\frac{1}{4}$ of an inch to 2 or 3 inches, and in general they close up and disappear as soon as the ether-sponge is removed or gets dry.

But not only was ether found to produce these effects, but also liquor ammoniæ, wood-spirit, alcohol, and naphtha, and, as I afterwards found, chloroform, benzole, bisulphide of carbon—in fact any substance that throws off vapour with facility—when a sponge wetted with one of these fluids was held over the film. The effects were not always so good as with ether, but each substance had peculiar features of its own, and no two films of different oils, &c. exhibited the same phenomena; indeed the films of the same substance would vary from day to day with varying temperatures of the air and other causes. It may be remarked that a sponge wetted with ammonia and held over the film often produced so violent an action as to break it up and scatter it about. It also forms with it a soapy compound which dissolves in the water. Ether-vapour may also produce as violent an action as ammonia. For example, a drop of oil of cinnamon produces on water a mottled film, reminding one of marbled paper. A sponge dipped in chloroform and held over the film, spreads it with a development of colour and incipient rings. The ether-sponge is then powerfully repulsive, spreading, breaking up and scattering the numerous discs into which a single drop of the oil forms. But the ammonia-sponge occasions a remarkable spreading, rapid motion, producing first coloured rings, and then a granular soapy structure, after which all further action ceases, from the film combining with the water.

In this way I accumulated a large number of results, which did not appear to throw much light upon the question as to what takes place between ether and water. There seemed to be a repulsive action of some kind, and I tried to measure it by means of a delicately hung torsion-balance of straw, making the straw carry a piece of filtering-paper which was saturated with water, while another piece of paper saturated with ether was brought up near to it; but I obtained no results in this way. I therefore tried the action of ether on a thin sheet of water just sufficient to cover the surface of clean mercury in a wine glass, or spread over a glass disc 5 or 6 inches in diameter with a ledge of

bees'-wax run round it. On presenting the ether-sponge to the centre of this sheet of water, the vapour drove away the water and left a circular dry space in the centre of the mercury or of the disc. This is a striking experiment, especially on the surface of the mercury, which shows the effect very well, and allows the thickness of the sheet of water to be somewhat greater than on the glass. The cohesion of the water is also well shown by its not closing up again when the ether-vapour is removed; but it forms a beautiful circular pupil with a convex surface towards the mercury. If, instead of the sponge, a dropping-tube containing ether be brought down to the surface of the water on the glass disc, the water will open as before; and on letting the ether fall the water will be still further repelled, so as to form a more convex ring round the liquid ether than it did around its vapour. Other volatile liquids produce different effects on this sheet of water. A single drop of creosote placed on the centre disperses the water, and leaves a long irregular portion of the glass dry. Several hundred drops of naphtha form a lenticular disc on the water without displacing it. A single drop of ether brought down upon it disperses both naphtha and water, and finds its way to the glass, leaving a convex-bounded ring which slowly closes in upon the dry space of glass. But the most remarkable result is with benzole: a disc of this being formed on the sheet of water, and the ether-sponge held over it, hollows it out into a thick ring and holds it in that state for some time. In fact there is a thick convex ring of benzole on water, the force which holds it open being the vapour of ether. Chloroform dropped on the sheet of water displaces it, and forms as it were a cavity, which it occupies by itself as in a pit of solid matter. The ammonia-sponge, when presented to the chloroform, drives small globules of the latter out of the cavity, and forms with the remainder a soapy looking compound which permanently excludes the water.

Results of this kind, however curious, only served to convince me that it is far more easy to multiply phenomena than to discover laws. Being strongly impressed with the idea of repulsion which these results seemed to favour, I tried the effects of heat; and instead of obtaining a clue to the explanation I was in search of, I extended the phenomena which we are now accustomed to call the "spheroidal condition" of matter. Boutigny's striking experiments had not then been contrived; and my first acquaintance with that class of phenomena was derived from Dumas's *Chimie Appliquée*, vol. i. p. 31, where is described the experiment of dropping water into a red-hot platinum crucible. I varied the experiment by dropping ether into it, and found it possible to accumulate a considerable quantity. I did the same

with spirits of wine, saline solutions, and even mercury. I changed the nature of the hot surface, and found that ether would roll about on hot mercury, hot oil, and hot water. I also found it perfectly easy to place a drop of water on rape or olive-oil heated to about 400° or 500°. All that was necessary was to deliver the water gently from a dropping-tube to the oil without any fall or splashing; it would then roll about for a considerable time. If ether were also placed on the hot oil, it would unite with the water and form a shell about it.

When M. Boutigny showed his remarkable experiments in your laboratory in 1845, you informed him of my results, and he admitted that they were quite new to him.

On trying some of the fixed oils on the surface of hot water and mercury, turpentine on hot sulphuric acid, &c., the single drop used for each experiment either became spheroidal, or flattened into a disc, the latter rotating on a vertical axis. Experiments of this kind were connected in my mind with the motions of camphor on the surface of water, as well as the agitation of ether and other liquids. I tried the effect of various vapours on camphor while rotating on water; and the results first suggested to me what I think is the key, if not the master-key, to these experiments; for, as I shall hereafter endeavour to show, electricity has some share in these results. A pellet of sponge saturated with benzole held over a rotating piece of camphor, had the effect of increasing the rotations of the smaller fragments to such an extent that the form of the camphor often became quite indistinct, and appeared as a mere cloudy haze. After an experiment of this kind, the morsel of camphor displays one or two brilliant points where the structure is altered and the light abundantly reflected. These points are the effect of solution. The benzole vapour seizes the camphor and begins to dissolve it; and during this action there is a contest between the cohesion of the camphor and the film formed by the condensed vapour of benzole, and the diffusive tendency of the water: there is a contest, in fact, between cohesion and adhesion. The formation of this film about the camphor may be plainly seen by holding another sponge dipped in chloroform instead of benzole over camphor: it first produces a rapid spinning, the effect of solution; but nearly as fast as the solution is formed the camphor is displaced by the water, and a solid opake crust of camphor is formed. Bisulphide of carbon held over the spinning camphor drives it about; and when a drop of that substance is placed on the water, it does not arrest the motions of the camphor, but follows it about. Persian naphtha causes the camphor to spin more rapidly; and a drop of that substance placed on the water will pursue the camphor with great swift-

ness, combine with it, and form a film which sometimes displays colour.

Taking *solution* as one of the keys, if not the master-key, to these experiments, and defining it as you do in your 'Chemical Physics' as a case of *adhesion* of a liquid to a solid (often overcoming cohesion), or of a liquid to a liquid, and moreover defining *saturation* as an equilibrium between the forces of adhesion and cohesion, I began to see more clearly the *rationale* of my numerous experiments. In order to test the adhesion view of the case, I looked about me for some fluid of nearly the same density as water, with but slight adhesion to it (that is, very sparingly soluble in it), but one which would readily saturate a moderate quantity of water (that is, a liquid whose cohesion would soon balance the adhesion of the water), so that whatever visible action might take place between the two would admit of being renewed from time to time by increasing the quantity of the water. After many trials I found exactly what was required in creosote. Although this substance is slightly heavier than water (spec. grav. 1·059), yet by carefully delivering a drop to the surface of water from the end of a glass rod it will not sink; the under surface of the drop will, however, present a convex bulge below the general surface of the water.

I wonder whether it ever occurred to a chemist to place a drop of creosote on the surface of water. It presents a most singular appearance: it flattens out into a disc with a silvery reflexion of the light, and sails about on the water with some speed, while it is all the time rapidly agitated with a motion that gives it the appearance of a living creature. Its edge vibrates with rapid crispations; it darts out small globules, which immediately begin a series of motions of rotation and translation on their own account. In the mean time a silvery film of creosote spreads over the surface of the water: the parent globule and the smaller globules become less energetic; they perform a number of motions among themselves, moving about in circular or curved paths, carefully avoiding each other, and reminding one of the water insects which may be seen sporting on the surface of a pond in summer. Sometimes the larger globules will remain still, and the smaller ones will rotate in little lakes, which they seem to clear out for themselves in the film to disport in. After some time they all come to rest; but again begin to move for a time, once more to come to rest, and, it may be, again to rotate.

Now there is evidently a struggle going on between the cohesion of the drop and the adhesion of the water. These two forces are so nicely balanced that it seems doubtful for a time which will prevail. The water tends to adhere to and diffuse the

creosote; the cohesion of the creosote tends to prevent this action, and the struggle between the two is manifested by a series of vibrations which take place at the edge of the disc; the creosote tends to spread, its cohesive force struggles to prevent the spreading. Small globules, however, are constantly being torn away from the parent drop, and as these spin round and disappear, they leave a film which gradually covers more or less of the surface of the water. The motions of the parent disc and of the globules cease; but as the film becomes dissolved by the water, the motions (now very slow ones) set in again with the formation of another film, which in its turn is dissolved. But if the quantity of water be small, the globules soon cease to be disturbed, since the water has become saturated, or the adhesion of the water balances the cohesion of the globule, and hence the quiescence.

When I showed you this experiment, it naturally struck you as a case of *solution*, and you inquired whether the motions of the disc would take place in a saturated solution. I had already ascertained that if, when one drop, or rather disc of creosote, is in rapid agitation, and also moving about on the water, a second drop be placed by its side, it stops the motions of the first disc, and is itself soon brought to rest. In other words, the water is sooner saturated. I further ascertained that if, when the discs of creosote are at rest in a small quantity of water—a wine-glass full, for example—the contents of such wine-glass were transferred to a half-pint goblet nearly full of water (the transfer being gently made, so as to keep the creosote on the surface), the motions of the discs will begin again with as much energy as before. When this half-pint of water is saturated and all motion has ceased, the motions will be renewed if the half-pint of water be poured into a large soda-water glass, two-thirds filled with water.

Thus, by increasing the quantity of water, we remove it further from the point of saturation, and favour the gyrations and other motions of the creosote. The same effect may be produced if (the quantity of water being fixed) we increase its solvent power. For example, a disc of creosote is very lively for some minutes on the surface of water; but as the latter becomes saturated the motions decline, and then cease. If we now touch the water with a glass rod wet with acetic acid, a new solvent power is conferred on the water, and the motions of the creosote set in again. So also these and other motions may be produced if we hold over the quiescent globule the vapours of substances in which creosote is soluble. The ether-sponge will cause the disc to display its crispations, and to dart out numerous globules. The ammonia sponge restores motion to

the globule while the latter is under the immediate influence of the gas. The benzole sponge acts with remarkable energy, causing the creosote to spread to the utmost verge of its cohesion, and then to split with a jerking kind of motion. Bisulphide of carbon has also a powerful action. The motions are also exceedingly curious when a drop of one of these substances is placed on the surface of water with the creosote, and about half an inch away from it. There is an interchange of action between them, an apparent repulsion, but in fact a contest between the solvent powers of the water and of the naphtha, &c. for the creosote. If a drop of bisulphide of carbon be placed near the creosote, the former remains lenticular, and does not evaporate so quickly as when placed alone on water; the creosote bombards it with a number of small globules, and is active only on the side nearest to it. If a drop of bisulphide be placed on either side of the creosote, the latter will carry on the bombardment from two sides. A drop of benzole is, in certain states of saturation of the water, so exceedingly active that it pursues the creosote, and attacks it with life-like motions. The latter darts about as if seeking to escape from it, and reminds one of an aquatic beetle pursuing its prey on the surface of a pond. These globules of creosote, benzole, &c., have thus a decided action on each other, but the lenticular discs which they form on water do not coalesce; they often attract each other with increasing velocity up to a certain point, and then repel each other, sailing slowly away until the force of attraction brings them near together again.

The phenomena may be further complicated by the action of vapours on the two dissimilar lenses. Thus creosote in the presence of a naphtha globule may be very lively, and the ether-sponge held over the creosote may break it up with a very decided action, and yet have little or no action on the naphtha.

I should detain you too long were I to describe the varied phenomena of this kind. They have an especial interest to me, because they led me to explain some of the other results partly by the same laws of solution. Thus one of my earliest experiments—the repulsion of lycopodium dust on the surface of water by ether—was not altogether a case of repulsion, but also of attraction—the attraction, in fact, of ether for water. But first, as to repulsion. That many of these phenomena display repulsion cannot be denied by any one who has witnessed them. The very circumstance of ether assuming so readily the vaporous state implies a strong degree of repulsion. As the ether leaves the saturated sponge, its comparatively feeble cohesion becomes changed into repulsion, *i. e.* the liquid becomes vapour, which

vapour being very heavy, falls down upon the excessively attenuated film, whose thickness must be measured by millionths of an inch. This heavy repulsive vapour then sweeps aside the film in a regular manner, producing in some cases a large central opening, and then a thinning off of the film, sufficient to allow of the interference of the light required for the phenomena of coloured rings. This descending vapour, moreover, meets with an ascending vapour from the film, and the contact of the two produces further complications in the way of chemical and electrical effects which I will not ask you to consider at present. I will only remark, *first*, that the electrical condition of these vapours is very decided, and must be considered before the explanation of these phenomena is at all complete; and *secondly*, that during solution, as of a salt in water, current electricity is (as I have reason to believe) largely concerned in the action and in the motions of the solid in the solvent. But my immediate point is with the mechanical action of this repulsive vapour from the wet sponge upon the film, the light powders, and the mobile discs of creosote, &c. There is a mechanical action about this vapour which goes some way to explain the production of the rings. A stream of gas from a gas-bottle produces them, and, what is equally remarkable, the vapour of oil of turpentine will repel a turpentine film. A drop of turpentine on water forms a good film, often at first without colour, but as it evaporates it displays colour. As soon, however, as the film is formed, if a sponge dipped into the same bottle from which the drop was taken be held over the film, it will spread out into very beautiful rings.

But now let us consider the influence of attraction. Admitting for the moment that water becomes saturated with one-eighth of its bulk of ether, it will be found that there is a very strong attraction between ether and its vapour for water up to this point. The water quickly becomes saturated, but the combination up to saturation is very energetic, especially at first. The great density of ether-vapour also assists the attractive force of the water; it falls down, the water seizes it, and disperses any dust or powder that may be in its way. If a film of oil or varnish be interposed, it begins to dissolve that, and thins it out to the gradually decreasing thicknesses necessary to the display of Newton's rings, often making a complete perforation, half an inch in diameter, in the film to get to the water, and showing the rings of colour at the inner edge of this perforation.

That this explanation is likely to be true the following experiment will prove:—Seven parts of water and one part of

ether were shaken up together and poured out into a small porcelain dish; the surface was dusted with lycopodium, and the ether-sponge presented to it: there was no action; the powder was not displaced or disturbed. The solution of ether was then boiled and filtered, and, when cold, the surface was again dusted with the powder. The ether-sponge now produced a repulsion of the powder, not so decidedly as with plain water, but still a good repulsion.

The solution of ether was also made to carry an oil-film. A drop of varnish formed an exquisite series of coloured rings, and the ether-sponge also displayed some very beautiful rings; but after a minute or two, when the adhesion between the solution and the film was complete, the ether-sponge was powerless.

It may also be mentioned that a vapour acts differently on the film according as it has a greater attraction for the water or for the film. If it has a strong attraction for the water, it will thin out and disperse the film. If its attraction is strong for the film, it will gather it up, thicken it, and deprive it of colour. Thus with a film of oil of lavender the ether scatters and disperses, while the benzole sponge thickens and attracts; in fact the benzole vapour condenses into little discs, which unite with the film. So also if a drop of oil of peppermint be placed on water, it spreads out into a honeycombed film displaying colour. If the ether-sponge be presented, the vapour pours down in a cataract and powerfully displaces the film (a very common effect of ether-vapour on films of the essential oils); whereas, if the turpentine sponge be held over it, the scattered parts of the film sail up to it, gather themselves together, and form a number of thickening lenticules.

I do not like to intrude further on your patience at present. Should this letter not disappoint the interest you have kindly expressed in this inquiry, I will trouble you with a second, and in the mean time subscribe myself,

Your attached friend,

CHARLES TOMLINSON.

King's College, London,
June 22, 1861.

XVIII. *On the Reduction of Observations of Underground Temperature; with Application to* Professor Forbes's *Edinburgh Observations, and the continued Calton Hill Series. By* Professor William Thomson, *F.R.S.*

[Concluded from p. 34.]

17. *APPLICATION to Thirteen Years' Observations* (1842–54) *at the Thermometric Station, Calton Hill.*—The observations on thermometers fixed by Professor Forbes at the different depths in the rock of Calton Hill, have been regularly continued weekly till the present time by the staff of the Royal Edinburgh Observatory, and regularly corrected to reduce to true temperatures of the bulbs, on the same system as before. Tables of these corrected observations, for the twelve years 1842 to 1854 inclusive, having been supplied to me through the kindness of Professor Piazzi Smyth, I have had the first five terms of the harmonic expression for each year determined in the following manner*:— In the first place, the observations were laid down graphically, and an interpolating curve drawn through the points, according to the method of Professor Forbes. The four curves thus obtained represent the history of the varying temperature, at the four different depths respectively, as completely and accurately as it can be inferred from the weekly observations. The space corresponding to each year was then divided into thirty-two equal parts (the first point of division being taken at the begining of the year), and the corresponding temperatures were taken from the curve. The coefficients of the double harmonic series (cosines and sines) for each year were calculated from these data, with the aid of the forms given by Mr. Archibald Smith, and published by the Board of Admiralty, for deducing the harmonic expression of the error of a ship's compass from observations on the thirty-two points. The general form of the harmonic expression being written thus—

$$V = A_0 + A_1 \cos 2\pi t + B_1 \sin 2\pi t + A_2 \cos 4\pi t + B_2 \sin 4\pi t + \&c.,$$

where V denotes the varying temperature to be expressed, and t the time, in terms of a year as unit. The following Table shows the results which were obtained, with the exception of the values of A_0:—

* The operations here described, involving, as may be conceived, no small amount of labour, were performed by Mr. D. M'Farlane, my laboratory assistant, and Mr. J. D. Everett, now Professor of Mathematics and Natural Philosophy in King's College, Windsor, N.S.

TABLE III.

Year.	Feet.	A_1.	B_1.	A_2.	B_2.	A_3.	B_3.	A_4.	B_4.
1842.	3	−6·19	−5·00	+·01	+ ·25	+ ·60	+·06	+·23	−·71
	6	−2·85	−4·80	−·15	+ ·03	+ ·10	+·10	+·12	−·26
	12	+ ·34	−2·73	−·12	− ·13	− ·08	−·04	+·01	−·04
	24	+ ·68	− ·14	·00	− ·07	− ·02	−·04	−·01	−·02
1843.	3	−4·75	−5·11	+·17	+ ·91	+1·23	+·30	+·79	−·17
	6	−1·63	−4·38	−·20	+ ·61	+ ·45	+·42	+·32	+·30
	12	+ ·83	−2·04	−·18	− ·08	− ·05	+·17	−·03	+·10
	24	+ ·62	+ ·12	·00	− ·02	− ·01	−·01	·00	·00
1844.	3	−5·29	−4·53	−·05	+ ·70	+ ·74	+·71	+·08	+·49
	6	−2·11	−4·09	+·22	+ ·50	+ ·20	+·50	−·06	+·20
	12	+ ·52	−2·15	+·18	+ ·05	+ ·11	+·13	−·05	−·01
	24	+ ·59	− ·02	−·03	− ·02	·00	−·03	−·01	−·02
1845.	3	−5·17	−5·01	−·17	+ ·56	+ ·67	+·29	−·28	+·02
	6	−2·02	−4·38	+·07	+ ·30	·00	+·18	−·04	−·08
	12	+ ·63	−2·15	+·12	+ ·06	− ·01	−·03	·00	+·02
	24	+ ·65	+ ·13	+·04	·00	+ ·01	+·02	+·01	+·02
1846.	3	−5·65	−5·17	+·03	+1·05	− ·86	+·64	+·00	−·49
	6	−2·37	−4·64	−·38	+ ·44	− ·63	−·39	−·11	−·22
	12	+ ·47	−2·70	−·30	− ·17	− ·14	−·45	·00	−·07
	24	+ ·64	− ·22	−·02	− ·17	+ ·03	−·11	−·03	−·06
1847.	3	−5·36	−5·31	+·69	+ ·24	− ·18	−·81	−·02	−·14
	6	−2·08	−4·58	+·18	+ ·32	+ ·11	−·39	−·05	−·04
	12	+ ·70	−2·37	−·03	+ ·17	+ ·12	+·14	+·03	+·02
	24	+ ·66	+ ·16	−·01	+ ·04	+ ·01	+·03	+·01	+·03
1848.	3	−5·83	−4·46	+·33	+ ·27	+ ·29	+·35	+·45	−·30
	6	−2·32	−4·16	+·13	+ ·27	+ ·02	+·23	+·28	+·09
	12	+ ·56	−2·15	+·04	+ ·16	− ·01	+·09	+·04	+·11
	24	+ ·66	+ ·10	−·01	+ ·03	·00	+·02	−·01	+·01
1849.	3	−4·56	−4·44	+·05	+1·14	− ·66	−·10	−·48	−·69
	6	−1·85	−3·97	−·20	+ ·45	− ·28	−·15	+·01	−·25
	12	+ ·49	−2·06	−·23	+ ·04	+ ·04	−·06	+·09	−·05
	24	+ ·57	+ ·03	·00	− ·02	+ ·01	+·02	·00	+·01
1850.	3	−5·40	−4·50	−·12	+ ·70	− ·54	−·82	−·15	−·42
	6	−2·43	−4·15	−·22	+ ·31	+ ·03	−·47	+·11	−·17
	12	+ ·17	−2·27	−·15	− ·04	− ·10	−·05	+·04	+·01
	24	+ ·61	− ·04	+·01	− ·03	+ ·01	·00	−·01	−·01
1851.	3	−4·18	−4·53	+·12	+ ·96	− ·09	+·31	+·22	+·18
	6	−1·65	−3·92	−·19	+ ·53	− ·18	+·07	−·03	+·14
	12	+ ·61	−1·99	−·22	+ ·01	− ·04	−·06	−·05	−·02
	24	+ ·56	+ ·02	+·01	− ·05	·00	−·01	−·14	−·01
1852.	3	−4·92	−4·80	+·20	+1·32	+ ·64	−·24	−·46	+·31
	6	−1·87	−4·25	−·23	+ ·71	+ ·15	+·10	−·31	−·02
	12	+ ·54	−2·24	−·26	+ ·05	+ ·01	+·09	−·01	−·07
	24	+ ·61	− ·03	−·12	− ·07	− ·01	−·04	·00	−·02
1853.	3	−5·08	−5·43	+·83	+ ·30	+ ·11	+·27	+·18	+·19
	6	−1·92	−4·57	+·38	+ ·41	− ·05	+·17	+·06	+·13
	12	+ ·76	−3·15	−·01	+ ·21	− ·01	·00	−·01	+·03
	24	+ ·62	+ ·18	−·39	+ ·03	·00	+·10	+·01	+·03
1854.	3	−5·69	−4·56	−·61	+ ·53	·00	−·15	+·15	−·20
	6	−2·48	−4·27	−·50	− ·01	·00	−·13	+·08	−·03
	12	+ ·42	−2·31	−·12	− ·21	+ ·02	−·03	+·02	+·01
	24	+ ·63	− ·03	+·02	− ·02	·00	−·01	−·01	−·01
Average for 13 years, 1842 to 1854.	3	−5·236	−4·835	+·114	+ ·687	+ ·150	+·0778	+·05462	−·14846
	6	−2·122	−4·320	−·0838	+ ·375	− ·00615	+·0185	+·02923	−·01615
	12	+ ·5415	−2 332	−·0985	+ ·00923	− ·01846	−·00778	+·006154	+·003078
	24	+ ·6231	− ·0200	−·0385	− ·0285	− ·00231	−·00462	−·01462	−·003846

The values which were found for A_0 should represent the annual mean temperatures. They differ slightly from the annual means shown in the Royal Observatory Report, which, derived as they are from a direct summation of all the weekly observations, must be more accurate. The variations, and the final average values of these annual means, present topics for investigation of the highest interest and importance, as I have remarked elsewhere (see British Association's Report, Section A, Glasgow, 1855); but as they do not belong to the special subject of the present paper, their consideration must be deferred to a future occasion.

18. *Theoretical Discussion.*—The mean value of the coefficients in the last line of the Table being obtained from so considerable a number of years, can be but very little influenced by irregularities from year to year, and must therefore correspond to harmonic functions for the different depths, which would express truly periodic variations of internal temperature consequent upon a continued periodical variation of temperature at the surface.

19. According to the principle of the superposition of thermal conductions, the difference between this continuous harmonic function of five terms for any one of the depths, and the actual temperature there at the corresponding time of each year, would be the real temperature consequent upon a certain real variation of superficial temperature. Hence the coefficients shown in the preceding Table afford the data, first by their mean values, to test the theory explained above for simple harmonic variations, and to estimate the conductivity of the soil or rock, as I propose now to do; and secondly, as I may attempt on a future occasion, to express analytically the residual variations which depend on the inequalities of climate from year to year, and to apply the mathematical theory of conduction to the non-periodic variations of internal temperature so expressed.

20. Let us accordingly now consider the complex harmonic functions corresponding to the mean coefficients of the preceding Table; and in the first place, let us reduce the double harmonic series in each case to series in each of which a single term represents the resultant simple harmonic variation of the period to which it corresponds, in the manner shown by the proposition and formulæ of § 3 above.

21. On looking to the annual and semiannual terms of the series so found, we see that their amplitudes diminish, and their epochs of maximum augment, with considerable regularity from the less to the greater depths. The following Table shows, for the annual terms, the logarithmic rate of diminution of the amplitudes, and the rate of retardation of the epoch between the points of observation in order of depth:—

TABLE IV.—Average of Thirteen Years, 1842 to 1854; Trap Rock of Calton Hill.

Depths below surface, in French feet.	Diminution of Napierian logarithm of amplitude, per French foot of descent.	Retardation of epoch in circular measure, per French foot of descent.
3 to 6 feet	·1310	·1233
6 to 12 „	·1163	·1140
12 to 24 „	·1121	·1145
3 to 24 „	·1160	·1156

22. The numbers here shown would all be the same if the conditions of uniformity supposed in the theoretical solution were fulfilled. Although, as in the previous comparisons, the agreement is on the whole better than might have been expected, there are certainly greater differences than can be attributed to errors of observation. Thus the means of the numbers in the two columns are for the three different intervals of depth in order as follows:—

	Mean deductions from amplitude and epoch.
3 to 6 feet	·127
6 to 12 „	·115
12 to 24 „	·113

numbers which seem to indicate an essential tendency to diminish at the greater depths. This tendency is shown very decidedly in each column separately; and it is also shown in each of the corresponding columns, in tables given above, of results derived from Professor Forbes's own series of a period of five years.

23. There can be no doubt that this discrepance is not attributable to errors of observation, and it must therefore be owing to deviation in the natural circumstances from those assumed for the foundation of the mathematical formula. In reality, none of the conditions assumed in Fourier's solution is rigorously fulfilled in the natural problem; and it becomes a most interesting subject for investigation to discover to what particular violation or violations of these conditions the remarkable and systematic difference discovered between the deductions from the formula and the results of observation is due. In the first place, the formula is strictly applicable only to periodic variations, and the natural variations of temperature are very far from being precisely periodic; but if we take the average annual variation through a sufficiently great number of years, it may be fairly presumed that irregularities from year to year will be eliminated: and that the discrepance we have now to explain does not de-

pend on residual inequalities of this kind seems certain, from the fact that it exists in the average of Professor Forbes's first five years' series no less decidedly than in that of the period of thirteen years following.

24. For the true explanation we must therefore look either to inequalities (formal or physical) in the surface at the locality, or to inequalities of physical character of the rock below. It may be remarked, in the first place, that if the rates of diminution of logarithmic amplitude and of retardation of epoch, while less, as they both are, at the greater depths, remained exactly equal to one another, the conductivity must obviously be greater, and the specific heat less in the same proportion inversely, at the greater depths. For in that case, all that would be necessary to reconcile the results of observation with Fourier's formula, would be to alter the scale of measurement of depths so as to give a nominally constant rate of diminution of the logarithmic amplitude and of the retardation of epoch; and the physical explanation would be, that thicker strata at the greater depths, and thinner strata at the less depths (all of equal horizontal area), have all equal conducting powers and equal thermal capacities*.

25. Now in reality, a portion, but only a portion, of the discrepance may be done away with in this manner; for while the logarithmic amplitudes and the epochs each experience a somewhat diminished rate of variation per French foot of descent at the greater depths, this diminution is much greater for the former than for the latter; so that, although the mean rates per foot on the whole 21 feet are as nearly as possible equal for the two (being ·1160 for the logarithmic amplitudes, and ·1156 for the epoch), the rate of variation of the logarithmic amplitude exceeds that of the epoch by about 6 per cent. on the average of the stratum of 3 to 6 feet; and falls short of it by somewhat more than 2 per cent. in the lower stratum, 12 to 24 feet. To find how much of the discrepance is to be explained by the variation of conductivity and specific heat in inverse proportion to one another at the different depths, we may take the mean of the

* The "conducting power" of a solid plate is an expression of great convenience, which I define as the quantity of heat which it conducts per unit of time when its two surfaces are permanently maintained at temperatures differing by unity. In terms of this definition, the specific conductivity of a substance may be defined as the conducting power per unit area of a plate of unit thickness. The conducting power of a plate is calculated by multiplying the number which measures the specific conductivity of its substance by its area, and dividing by its thickness.

The *thermal capacity of a body* may be defined as the quantity of heat required to raise its mass a unit (or one degree) of temperature. The specific heat of a substance is the thermal capacity of a unit quantity of it, which may be either a unit of weight or a unit of bulk.

rates of variation of logarithmic amplitude and of epoch at each depth, and alter the scale of longitudinal reckoning downwards, so as to reduce the numerical measures of these rates to equality. This, however, we shall not do in either the five years' or the thirteen years' term, which we have hitherto considered separately, but for a harmonic annual variation representing the average of the whole eighteen years 1837 to 1854.

26. By taking for each depth the coefficients A_1, B_1 (not explicitly shown above), derived from the first five years' average, and multiplying by 5; taking similarly the coefficients A_1, B_1 for the succeeding thirteen years' average, and multiplying by 13; adding each of the former products to the corresponding one of the latter, and dividing by 18; we obtain, as the proper average for the whole eighteen years, the values shown in the following Table, in the columns headed A_1, B_1. The amplitudes and epochs shown in the next columns are deduced from these by the formulæ $\sqrt{(A_1^2+B_1^2)}$ and $\tan^{-1}\frac{B_1}{A_1}$ respectively:—

TABLE V.—Annual Harmonic Variation of Temperature in Calton Hill, from 1837 to 1844 inclusive.

Depths.	A_1 in degrees Fahr.	B_1 in degrees Fahr.	Amplitudes in degrees Fahr.	Epochs in degrees and minutes.
3 feet	$-5^\circ\cdot184$	$-4^\circ\cdot989$	$7^\circ\cdot1949$	$223^\circ\ 54'$
6 „	$-2\cdot080$	$-4\cdot416$	$4\cdot8812$	244 47
12 „	$+\ \cdot5961$	$-2\cdot3345$	$2\cdot4094$	284 19
24 „	$+\ \cdot6311$	$+\ \cdot0306$	$\cdot6319$	362 47

From these, as before, for ten terms of five years and of thirteen years separately, we deduce the following:—

TABLE VI.—Average of Eighteen Years, 1837 to 1844; Trap Rock of Calton Hill.

Depths below surface in French feet.	Diminution of logarithmic amplitude, per French foot of descent.	Retardation of epoch in circular measure, per French foot of descent.
3 to 6 feet	·1286	·1215
6 to 12 „	·1177	·1150
12 to 24 „	·1115	·1141
3 to 24 „	·1157	·1154

27. Hence we have as final means, of effects on logarithmic amplitudes and on epochs, for the average annual variation on the whole period of eighteen years,—

1. From depth 3 feet to 6 feet . . . ·1250
2. „ 6 „ 12 „ . . . ·1163
3. „ 12 „ 24 „ . . . ·1128

If now, in accordance with the proposed plan, we measure depths, not in constant units of length, but in terms of thicknesses corresponding to equal conducting powers and thermal capacities, and if we continue to designate the thickness of the first stratum by its number 3 of French feet, our reckoning for the positions of the different thermometers will stand as follows:—

TABLE VII.

Thermometers numbered downwards.	Depths in true French feet below No. 1.	Depths in terms of conductive equivalents.
I.	0	0
II.	3	3
III.	9	$3+\frac{\cdot1163}{\cdot1250}\times6=8\cdot58$
IV.	21	$8\cdot58+\frac{\cdot1128}{\cdot1250}\times12=19\cdot41$

According to this way of reckoning depths, we have the following rates of variation of the logarithmic amplitudes, and of the epochs separately, reduced from the previously stated means for the whole period of eighteen years:—

TABLE VIII.

Portions of rock.	Rates of diminution of logarithmic amplitude per French foot, and conductive equivalents.	Rate of retardation of epoch per French foot, and conductive equivalents.
Between thermometers Nos. I. and II.	·1286	·1215
„ „ II. and III.	·1265	·1236
„ „ III. and IV.	·1236	·1264
Between thermometers Nos. I. and IV.	·1252	·1248

28. Comparing this Table with the preceding Table VI., we see that the discrepancies are very much diminished; and we cannot doubt that the conductive power of the rock is less in the lower parts of the rock, and that the amount of the variation is approximately represented by Table VII. We have, however, in Table VIII. still too great discrepancies to allow us to consider variation in the value of kc as the only appreciable deviation from Fourier's conditions of uniformity.

29. In endeavouring to find whether these residual discre-

pancies are owing to variations of k and c not in inverse proportion one to the other, I have taken Fourier's equation

$$c\frac{dv}{dt}=k\frac{d^2v}{dx^2}+\frac{dk}{dx}\frac{dv}{dx},$$

where v denotes the temperature at time t, and at a distance x from an isothermal plane of reference (a horizontal plane through thermometer No. 1., for instance); k the conductivity, varying with x; and c the capacity for heat of a unit of volume, which may also vary with x. In this equation I have taken

$$v=ae^{-P}\cos\left(\frac{2\pi t}{T}-Q\right),$$

where P and Q are functions of x, assumed so as to express, as nearly as may be, the logarithmic amplitudes, and the epochs, deduced from observation. I have thus obtained two equations of condition, from which I have determined k and c, as functions of x. The problem of finding what must be the conductivity and the specific heat at different depths below the surface, in order that, with all the other conditions of uniformity perfectly fulfilled, the annual harmonic variation may be exactly that which we have found on the average of the eighteen years' term at Calton Hill, is thus solved. The result is, however, far from satisfactory. The small variations in the values of P and Q which we have found in the representation of the observed temperatures require very large and seemingly unnatural variations in the values of k and c.

30. I can only infer that the residual discrepancies from Fourier's formula shown in Table VIII. are not with any probability attributable to variations of conductivity and specific heat in the rock, and conclude that they are to be explained by irregularities, physical and formal, in the surface. It is possible, indeed, that thermometric errors may have considerable influence, since there is necessarily some uncertainty in the corrections estimated for the temperatures of the different portions of the columns of liquid above the bulbs; and before putting much confidence in the discrepancies we have found as true expressions of the deviations in the natural circumstances from Fourier's conditions, a careful estimate of the probable or possible amount of error in the observed temperatures should be made. That even with perfect *data* of observation as great discrepancies should still be found in final reductions such as we have made, need not be unexpected when we consider the nature of the locality, which is described by Professor Forbes in the following terms:—

The position chosen for placing the thermometer was below the surface "in the Observatory enclosure on the Calton Hill, at

a height of 350 feet above the sea. The rock is a porphyritic trap, with a somewhat earthy basis, dull and tough fracture. *The exact position is a few yards east of the little transit house.* There are *also other buildings in the neighbourhood.* The ground rises slightly to the east, and *falls abruptly to the west at a distance of fifteen yards.* The immediate surface is flat, *partly covered with grass, partly with gravel**."

I have marked by italics those passages which describe circumstances such as it appears to me might account for the discrepancies in question.

31. *Application to Semiannual Harmonic Terms.*—The harmonic expressions given above (§ 15) for the average periodic variations for the three stations of Professor Forbes's original series of five years' observations, contain semiannual terms which are obviously not in accordance with theory. The retardations of epochs and the diminutions of amplitudes are, on the whole, too irregular to be reconcileable by any supposition as to the conductivities and specific heat of the soils and rocks involved, or as to the possible effects of irregularity of surface; and in two of the three stations the amplitude of the semiannual term is actually greater as found for the six-feet deep than for the three-feet deep thermometer, which is clearly an impossible result. The careful manner in which the observations have been made and corrected seems to preclude the supposition that these discrepancies, especially for the three-feet and six-feet thermometers, for which the amplitudes of the semiannual terms are from ·28° to ·74° (corresponding to variations of double those amounts, or from ·56° to 1°·48), can be attributed to errors in the *data*. It must be concluded, therefore, that the semiannual terms of those expressions do not represent any truly periodic elements of variation, and that they rather depend on irregularities of temperature in the individual years of the term of observation. Hence, until methods for investigating the conduction inwards of non-periodic variations of temperature are applied, we cannot consider that the special features of the progress of temperature during the five years' period at the three stations, from which our apparent semiannual terms have been derived, have been theoretically analysed. But, as we have seen, every irregularity depending on individual years is perfectly eliminated when the average annual variation over a sufficiently great number of years is taken. Hence it becomes interesting to examine particularly the semiannual terms for the eighteen years' average of the Calton Hill thermometers, which we now proceed to do.

* Professor Forbes "On the Temperature of the Earth," Trans. Roy. Soc. Edinb. 1846, p. 194.

32. Calculating as above (§ 26), for the coefficients A_1, B_1, the average values of A_2 and B_2, from Professor Forbes's results for his first five years' term, and from the averages for the next thirteen years shown in Table III. above, we find the values of A_2 and B_2 shown in the following Table. The amplitudes and epochs are deduced as usual by the formulæ $\sqrt{(A_2^2+B_2^2)}$ and $\tan^{-1}\frac{B_2}{A_2}$. These reductions I only make for the three-feet deep and the six-feet deep thermometers, since, for the two others, as may be judged by looking at the thirteen years' average shown in the former Table, the amounts of the semiannual variation do not exceed the probable errors in the data of observation sufficiently to allow us to draw any reliable conclusions from their apparent values.

Table IX.—Average Semiannual Harmonic Term, from Eighteen Years' Observations at Calton Hill.

Depths below surface, in French feet.	A_2 in degrees Fahr.	B_2 in degrees Fahr.	Amplitudes in degrees Fahr.	Epochs in degrees and minutes.
3 feet.	°·1518	°·5842	°·604	75° 26′
6 feet.	·0461	·3911	·394	96 43

The ratio of diminution of the amplitude here is $\frac{\cdot 604}{\cdot 394}$, or 1·53, of which the Napierian logarithm is ·426. Dividing this by 3, we find

·142

as the rate of diminution of the logarithmic amplitude per French foot of descent.

The retardation of epoch shown is 21° 17′; and therefore the retardation per French foot of descent is 7° 6′, or, in circular measure,

·1239.

If the data were perfect for a periodical variation, and the conditions of uniformity supposed in Fourier's solution were fulfilled, these two numbers would agree, and each would be equal to $\sqrt{\frac{2\pi k}{c}}$. Hence, dividing them each by $\sqrt{2}$, we find

Apparent values of $\sqrt{\frac{\pi c}{k}}$.

·100 (by amplitudes).
·0877 (by epochs).

The true value of $\sqrt{\frac{\pi c}{k}}$ must, as we have seen, be ·116, to a very close degree of approximation.

33. When we consider the character of the reduction we have made, and remember that the data were such as to give no semblance of a theoretical agreement when the first five years' term of observations was taken separately, we may be well satisfied with the approach to agreement presented by these results, depending as they do on only eighteen years in all, and we may expect that, when the average is of a still larger term of observation, the discrepancies will be much diminished. In the mean time we may regard the semiannual term we have found for the three-feet deep thermometer as representing a true feature of the yearly vicissitude; and it will surely be interesting to find whether it is a constant feature for the locality of Edinburgh, to be reproduced on averages of subsequent terms of observation.

34. It may be remarked that the nearer to the equator is the locality, the greater relatively will be the semiannual term; that within the tropics the semiannual term may predominate, except at great depths; and that at the equator the tendency is for the annual term to disappear altogether, and to leave a semiannual term as the first in a harmonic expression of the yearly vicissitude of temperature. The facilities which underground observation affords for the analysis of periodic variations of temperature when the method of reduction which I have adopted is followed, will, it is to be hoped, induce those who have made similar observations in other localities to apply the same kind of analysis to their results; and it is much to be desired that the system of observing temperatures at two, if not more depths below the surface may be generally adopted at all meteorological stations, as it will be a most valuable means for investigating the harmonic composition of the annual vicissitudes.

III. *Deduction of Conductivities.*

35. Notwithstanding the difficulty we have seen must attend any attempt to investigate all the circumstances which must be understood in order to reconcile perfectly the observed results with theory, the general agreement which we have found is quite sufficient to allow us to form a very close estimate of the ratio of the conductivity of the rock to its specific heat per unit of bulk. Thus, according to the means deduced from the whole period of eighteen years' observation, the average rate of variation of the logarithmic amplitude of the annual term through the whole space of twenty-one feet is ·1157, and of the epoch of the same term, ·1154. The mean of these, or ·1156, can differ but very little

from the true average value of $\sqrt{\frac{\pi c}{k}}$ for the portion of rock between the extreme thermometers.

36. Dividing π by the square of the reciprocal of this number, we find 235·1 as the value of $\frac{k}{c}$, or, as we may call it, the conductivity of the rock in terms of the thermal capacity of a cubic foot of its own substance. In other words, we infer that all the heat conducted in a year (the unit of time) across each square foot of a plate one French foot thick, with its two sides maintained constantly at temperatures differing by 1°, would, if applied to raise the temperature of portions of the rock itself, produce a rise of 1° in 235 cubic feet. As it is difficult (although by no means impossible) to imagine circumstances in which the heat, regularly conducted through a stratum maintained, with its two sides, at perfectly constant temperatures, could be applied to *raise* the temperatures of other portions of the same substance, we may vary the statement of the preceding result, and obtain the following completely realizable illustration.

37. Let a large plate of the rock, everywhere one French foot thick, have every part of one of its sides (which, to avoid circumlocution, we shall call its lower side) maintained at one constant temperature, and let portions of homogeneous substance, at a temperature 1° lower, be continually placed in contact with the upper surface, and removed to be replaced by other homogeneous portions at the same lower temperature, as soon as the temperature of the matter actually thus applied rises in temperature by $\frac{1}{1000}$ of a degree. If this process is continued for a year, the whole quantity of the refrigerating matter thus used to carry away the heat conducted through the stratum must amount to 235,000 cubic feet for each square foot of area, which will be at the rate of ·00745 of a cubic foot per second. We may therefore imagine the process as effected by applying an extra stratum ·00745 of a foot thick every second of time. This extra stratum, after lying in contact for one second, will have risen in temperature by $\frac{1}{1000}$ of a degree. By means of the information contained in this apparently unpractical statement, many interesting problems may be practically solved, as I hope to show in a subsequent communication.

38. The value of $\sqrt{\frac{\pi c}{k}}$, derived from the whole eighteen years' period of observation (·1156), differs so little from that (·1154) found previously (§ 16) from Professor Forbes's observations and reductions of the first five of the years, that we may feel much confidence in the accuracy of the values ·1098 and

·06744, which, from his five years' data alone, we found (§ 16) for the corresponding constant with reference to the sand at the Experimental Garden and the sandstone of Craigleith Quarry. From them, calculating as above (§ 36), we find 260·5 and 690·7 as the values of $\frac{k}{c}$ for the terrestrial substances of these localities respectively,—results of which the meaning is illustrated by the statements of §§ 36 and 37.

39. To deduce the conductivities of the strata in terms of uniform thermal units, Professor Forbes had the "specific heats" of the substances determined experimentally by M. Regnault. The results, multiplied by the specific gravities, gave for the thermal capacities of portions of the three substances, in terms of that of an equal bulk of water, the values ·5283, ·3006, and ·4623 respectively. Now these must be the values of c if the thermal unit in which k is measured is the thermal capacity of a French cubic foot of water. Multiplying the values of $\frac{k}{c}$ found above by these values of c, we find for k the following values:—

Trap-rock of Calton Hill.	Sand of Experimental Gardens.	Sandstone of Craigleith.
124·2	78·31	319·3

The values found by Professor Forbes were—

111·2	82·6	298·3

Although many comparisons have been made between the conducting powers of different substances, scarcely any data as to thermal conductivity in absolute measure have been hitherto published, except those of Professor Forbes, and probably none approaching to their accuracy. The slightly different numbers to which we have been led by the preceding investigation are no doubt still more accurate.

40. To reduce these results to any other scale of linear measurement, we must clearly alter them in the inverse ratio of the square of the absolute lengths chosen for the units*. The

* Because the absolute amount of heat flowing through the plate across equal areas will be inversely as the thickness of the plate, and the effect of equal quantities of heat in raising the temperature of equal areas of the water will be inversely as the depth of the water. The same thing may be perhaps more easily seen by referring to the elementary definition of thermal conductivity (footnote to § 11 above). The absolute quantity of heat conducted across unit area of a plate of unit thickness, with its two sides maintained at temperatures differing by always the same amount, will be directly as the areas, and inversely as the thickness, and therefore simply as the absolute length chosen for unity. But the thermal unit in which these quantities are measured, being the capacity of a unit bulk of water, is

length of a French foot being 1·06575 of the British standard foot, we must therefore multiply the preceding numbers by 1·13581 to reduce them to convenient terms.

41. We may, lastly, express them in terms of the most common unit, which is the quantity of heat required to raise the temperature of a grain of water by 1°; and to do this we have only to multiply each of them by $7000 \times 62{\cdot}447$, being the weight of a cubic foot in grains.

42. The following Table contains a summary of our results as to conductivity expressed in several different ways, one or other of which will generally be found convenient:—

TABLE X.—Thermal Conductivities of Edinburgh Strata, in British Absolute Units [Unit of Length, the English Foot].

Description of terrestrial substance.	Conductivities in terms of thermal capacity of unit bulk of substance $\left(\frac{k}{c}\right)$.			Conductivities in terms of thermal capacity of unit bulk of water (k).			Conductivities in terms of thermal capacity of one grain of water.
	Per ann.	Per 24 h.	Per second.	Per ann.	Per 24 h.	Per second.	Per second.
Trap-rock of Calton Hill.	267·0	·7310	·000008461	141·1	·3863	·000004471	1·9544
Sand of Experimental Gardens...	295·9	·8100	·000009375	88·9	·2435	·000002818	1·2319
Sandstone of Craigleith Quarry ...	784·5	2·1478	·00002486	362·7	·9929	·00001149	5·0225

43. The statements (§§ 36 and 37) by which the signification of $\frac{k}{c}$ has been defined and illustrated, require only to have *cubic feet of water* substituted for *cubic feet of rock*, in their calorimetric specifications, to be applicable similarly to define and illustrate the meaning of the conductivity denoted by k. The fluidity of the water allows a modified and somewhat simpler explanation, equivalent to that of § 36, to be now given as follows:—

44. If a long rectangular plate of rock one foot thick, in a position slightly inclined to the horizontal, have water one foot deep flowing over it in a direction parallel to its length, and if the lower surface of the plate be everywhere kept 1° higher in temperature than the upper, the water must flow at the rate of k times the length of the plate per unit of time in order that the heat conducted through the plate may raise it just 1° in tempe-

directly as the cube of the unit length, and therefore the numbers expressing the quantities of heat compared will be inversely as the cubes of the lengths chosen for unity, and directly as these simple lengths; that is to say, finally, they will be inversely as the squares of these lengths.

rature in its flow over the whole length. [It must be understood here that the plate becomes warmer, on the whole, under the lower parts of the stream of water, its upper surface being everywhere at the same temperature as the water in contact with it, while its lower surface is, by hypothesis, at a temperature 1° higher.] If, for instance, the plate be of Calton Hill trap-rock, the water must, according to the result we have found, flow at the rate of 141·1 times its length in a year, or of ·3863 of its length in twenty-four hours, to be raised just 1° in temperature in flowing over it. Thus, water one French foot deep, flowing over a plane bed of such rock at the rate of ·3863 of a mile in twenty-four hours, will in flowing one mile have its temperature raised 1° by heat conducted through the plate. The rates required to fulfil similar conditions for the sand of the Experimental Gardens and the sandstone of Craigleith Quarry are similarly found to be ·2435 of the length and ·9929 of the length in twenty-four hours.

XIX. *Chemical Notices from Foreign Journals.* *By* E. Atkinson, *Ph.D., F.C.S.*

[Continued from p. 62.]

LOURENCO* has succeeded in converting glycerine into propylic glycol, and glycol into ordinary alcohol. The formula of monohydrochloric glycerine only differs from that of propylic glycol by containing chlorine in the place of an atom of hydrogen. This relation, as well as that between monohydrochloric glycol and the corresponding monoatomic alcohol, is indicated in the following formulæ:—

$C^3 H^7 Cl O^2$	$C^3 H^8 O^2$
Monohydrochloric glycerine.	Propylic glycol.
$C^3 H^7 Cl O^4$	$C^3 H^8 O^4$
Monohydrochloric glycol.	Propylic alcohol.
$C^2 H^5 Cl O^4$	$C^2 H^6 O^4$
Hydrochloric glycol.	Alcohol.

By treating these hydrochloric ethers with nascent hydrogen, this chlorine is removed and replaced by hydrogen.

When monohydrochloric glycerine, diluted with its volume of water, was placed in contact with excess of sodium-amalgam, and the mixture left at the ordinary temperature, the amalgam was slowly decomposed with a slight disengagement of hydrogen, and formation of an abundant deposit of chloride of sodium.

* *Comptes Rendus*, May 20, 1861.

The reaction was terminated in two or three days; the contents were digested with strong alcohol, filtered, and the alkaline liquor neutralized with acetic acid and distilled. When the water and alcohol had passed over, the thermometer rapidly rose, and between 180° and 190° a colourless oily liquid distilled over, which was found to have all the properties, physical and chemical, of Wurtz's propylglycol.

Ordinary glycol was converted into alcohol by an analogous process. Hydrochloric glycol, diluted with half its volume of water, was mixed with excess of sodium-amalgam. When left at the ordinary temperature, it became converted into ordinary alcohol; at a higher temperature the character of the reaction was different, some oxide of ethylene being formed. The product of the reaction was distilled off in the water-bath, and the distillate, dried by carbonate of potash and by caustic baryta, was found to have the composition and properties of ordinary alcohol.

It is exceedingly probable that the transformation of propylic glycol into propylic alcohol would take place just in the same way.

Strecker has published* a very interesting investigation on the relations between guanine, xanthine, caffeine, theobromine, and creatinine.

He describes a modification of the method of preparing guanine, and also describes some compounds which it forms with nitrate of silver and with baryta, and which are analogous to the compounds with sarcine and xanthine.

Unger found, by oxidizing guanine with chlorate of potash, that an acid was formed which had the formula $C^{10}H^5N^4O^9$, and which he named *parrie* acid. These experiments have been repeated by Strecker, who has found that the acid in question is parabanic acid, $C^6H^2N^2O^6$. The mother-liquor contains, in addition to this, the hydrochlorate of a new organic base, which he calls *guanidine*. It is a strongly alkaline body, which forms neutral crystalline salts with most acids. The free base is a crystalline mass with a caustic taste, which rapidly attracts water and carbonic acid from the air, and thus is unfitted for direct analysis. Its formula, $C^2H^5N^3$, was determined from the analysis of its platinum-salt, $C^2H^5N^3HCl,PtCl^2$; its carbonate, $C^2H^5N^3HO,CO^2$; and its oxalate, $C^2H^5N^32HO,C^4H^2O^8$.

The decomposition of guanine may be thus expressed:—

$$C^{10}H^5N^5O^2+2HO+6O=C^6H^2N^2O^6+C^2H^5N^3+2CO^2.$$

Guanine. Parabanic acid. Guanidine.

* Liebig's *Annalen*, May 1861.

When treated with nitric acid, guanidine is oxidized into urea:

$$\underset{\text{Guanidine.}}{C^2H^5N^3} + 2HO = \underset{\text{Urea.}}{C^2H^4N^2O^2} + NH^3.$$

When heated, guanidine yields products analogous to those from mellone; it stands in close relation to cyanamide, and may be considered as *cyanodiamine*, $C^2H^5N^3 = \left.\begin{matrix} C^2N \\ H^2 \\ H^3 \end{matrix}\right\}N$.

In its decompositions guanine is closely allied to creatine; for Dessaignes found that this body, when oxidized, is resolved into oxalic acid and methyluramine, $C^4H^7N^3$, which is nothing more than methylguanidine, $\left.\begin{matrix} C^2N \\ C^2H^3 \\ H^4 \end{matrix}\right\}N^2$. And under other circumstances Dessaignes found that creatine yields an acid, $C^8H^4N^2O^6$, which appears to be a methylparabanic acid, judging from a comparison of the properties and of the formulæ of the two acids,

$$\underset{\text{Parabanic acid.}}{\left.\begin{matrix} C^4O^4 \\ C^2O^2 \\ H^2 \end{matrix}\right\}N^2} \qquad \underset{\text{Methylparabanic acid.}}{\left.\begin{matrix} C^4O^4 \\ C^2O^2 \\ C^2H^3.H \end{matrix}\right\}N^2}$$

From its decompositions, creatine may be regarded as composed of cyanamide and of methylglycocol (sarcosine); and its relations to creatinine and guanine are evident from a comparison of the formulæ

$$\underset{\text{Creatine.}}{\left.\begin{matrix} C^2N \\ H^2 \end{matrix}\right\}N \\ C^4H^2O^2 \\ \left.\begin{matrix} C^2H^3 \\ H^2 \end{matrix}\right\}N \\ O^2} = \underset{\text{Creatinine.}}{\left.\begin{matrix} C^2N \\ C^4H^2O^2 \\ C^2H^3 \\ H^4 \end{matrix}\right\}N^2 \\ O^2} \qquad \underset{\text{Guanine.}}{\left.\begin{matrix} C^2N \\ C^4H^2O^2 \\ C^2H^3 \\ H^2 \end{matrix}\right\}N^2} \qquad \left.\begin{matrix} C^4H^2 \\ C^2N \\ C^4H^2O^2 \\ H^3 \end{matrix}\right\}N^2;$$

that is, guanine may be regarded as creatinine containing an atom of hydrogen in the place of methyle, and containing besides 2 equivs. of cyanogen, in which it is analogous to a number of organic bases, such as cyaniline, cyanocodeine, &c.

Strecker has also investigated several points in reference to the artificial xanthine which he prepared*, which prove that it is identical with the natural product, and not merely isomeric, as had been suggested. He finds that xanthine is soluble in 570 parts of boiling, and in 2120 parts of cold water.

From its formula, xanthine may be considered as belonging to an homologous series along with theobromine and caffeine, as

* Phil. Mag. vol. xviii. p. 135.

their formulæ only differ by $n C^2 H^2$,

$C^{10} H^4 N^4 O^4$	$C^{14} H^8 N^4 O^4$	$C^{16} H^{10} N^4 O^4$
Xanthine.	Theobromine.	Caffeine.

Strecker has indeed shown that theobromine may be converted into caffeine, to which it bears the same relation as aniline to methylaniline. When theobromine is dissolved in ammonia, and nitrate of silver added, a precipitate forms which readily dissolves in warm ammonia. On boiling this, a granular crystalline precipitate is deposited, which is theobromine-silver. When this is treated with iodide of methyle, iodide of silver is formed, and a crystalline body, which is caffeine:

$$C^{14} H^7 Ag N^4 O^4 + C^2 H^3 I = C^{16} H^{10} N^4 O^4 + Ag I.$$

Theobromine-silver. Iodide of methyle. Caffeine.

Strecker attempted a similar transformation of xanthine into theobromine by treating the silver compound of xanthine, which contains 2 equivs. of silver, $C^{10} H^2 Ag^2 N^2 O^4$, with 2 equivs. of iodide of methyle. He obtained a body of the same composition as theobromine, but which is simply isomeric, and not identical with it.

Gerhardt had already pointed out that *cholestrophane*, a product of the decomposition of caffeine, might be regarded as a bimethylated parabanic acid; and from this point of view Hlasiwetz endeavoured, but without success, to convert parabanic acid into this body. Strecker, by treating parabanate of silver with iodide of methyle, has succeeded in effecting the change; that is, by replacing 2 equivs. of hydrogen in parabanic acid by 2 equivs. of methyle, cholestrophane is obtained, as is indicated in the formula

$\left.\begin{matrix} C^4 O^4 \\ C^2 O^2 \\ H^2 \end{matrix}\right\} N^2$	$\left.\begin{matrix} C^4 O^4 \\ C^2 O^2 \\ 2C^2 H^3 \end{matrix}\right\} N^2$
Parabanic acid.	Cholestrophane.

Hence, while the oxidation of uric acid, of guanine, and of xanthine gives parabanic acid, from caffeine, dimethylparabanic acid is obtained; and from creatine, methylparabanic acid.

Stas* has published the results of an investigation of the atomic weights of the elements which has occupied him during several years. His object was to subject Prout's hypothesis, that all the atomic weights of the elements are multiples by whole numbers of that of hydrogen as unity, to a more rigorous scrutiny, and to ascertain whether there was in fact a common divi-

* *Bulletin de l'Academie Royale de Belgique*, sér. 2. vol. x. No. 8. Liebig's *Annalen*, Supplement, May 1861.

sor for the atomic weights. The original memoir must be consulted for an account of the great labour and pains taken to ensure the purity of the substances used, accuracy in the weighings, and the exclusion of all sources of error from the apparatus employed. In these respects the memoir is probably one of the most important which has ever appeared. In one point his determinations differ from those of preceding chemists—that is, in the quantities taken, which are very considerably larger than those usually employed; the balances used were on a corresponding scale, and of the most perfect construction. It may be mentioned that he found any kind of glass attacked when heated directly by flame; but that, when protected by a coating of charcoal or of magnesia, it could be heated to softening without undergoing any alteration in weight. In all cases, where practicable, Stas used vessels of platinum.

The present communication refers to the atomic weights of nitrogen, chlorine, sulphur, potassium, sodium, and silver, and their relation to the atomic weight of hydrogen. The author made for this purpose the synthesis of the following substances—chloride of silver, sulphide of silver, nitrate of silver, nitrate of lead, sulphate of lead; the analysis of chlorate of potash, and sulphate of silver; and he determined the relations between the atomic weights of the following substances:—silver and chloride of potassium, silver and chloride of ammonium, silver and chloride of sodium, nitrate of silver and chloride of potassium, nitrate of silver and chloride of ammonium.

A most essential point was the preparation of pure metallic silver. Several different methods were used, one of which consisted in digesting a dilute solution of silver with finely divided phosphorus. This action is slow, but the metal, after having been digested with ammonia, is quite pure.

The synthesis of chloride of silver was effected by heating pure silver in a current of chlorine, and expelling the excess of chlorine by dry air. In three experiments there were obtained for 100 parts of silver, 132·841, 132·843, and 132·843 respectively of chloride of silver. Chloride of silver was also prepared by passing hydrochloric acid gas over the surface of a solution of silver in acid. The precipitate was dried in the same vessel, and fused in an atmosphere of hydrochloric acid. Two experiments of this kind, one of which was made with 400 grammes of silver, gave respectively 132·849 and 132·846 of chloride of silver from 100 parts of silver. In another series the silver was dissolved in nitric acid, and precipitated by a feeble excess of hydrochloric acid, washed, and fused in a current of the gas. This gave for 100 of silver 132·846 of chloride. In the last series, solution of silver was precipitated by chloride of ammonium, the precipi-

tate washed, dried, and fused in hydrochloric acid. From 100 of silver there were obtained 132·848 and 132·8417 of chloride of silver.

The synthesis of nitrate of silver was effected by dissolving pure silver in nitric acid, evaporating to dryness in a Bohemian-glass vessel, heating in a current of dry air, and fusion till constant. The mean of eight experiments gave for the relation between silver and nitrate of silver

$$100 : 157{\cdot}492.$$

Sulphuret of silver was prepared by heating a known weight of silver, either in the vapour of pure sulphur or in sulphuretted hydrogen gas. In the mean of five experiments, 100 parts of silver gave 114·8522 of sulphuret.

The method for estimating the relation between the equivalents of silver and of the chlorides of sodium, ammonium, and potassium, was that of Gay-Lussac. It consisted in dissolving a known weight of silver in nitric acid, and adding an equivalent of the chloride in question; the equivalents being calculated according to Prout's law. The excess of silver after precipitation, estimated by standard solutions, gave the required relations.

The chloride of potassium was prepared either from carefully purified chlorate of potash, from platinochloride of potassium, from nitrate of potash, or from tartrate of potash. The mean of 19 experiments with quantities of pure silver varying from 3 to 32 grms., gave for the relation,

$$Ag : K\,Cl = 100 : 69{\cdot}103.$$

Fourteen similar experiments with chloride of sodium led to the relation

$$Ag : Na\,Cl = 100 : 54{\cdot}2078.$$

The relation between the equivalent of silver and that of chloride of ammonium, as obtained from ten experiments, was

$$Ag : NH^4\,Cl = 100 : 49{\cdot}5944.$$

For the relation between nitrate of silver and chloride of potassium the numbers were

$$AgO\,NO^5 : K\,Cl = 100 : 43{\cdot}8758;$$

and for the relation between nitrate of silver and chloride of ammonium,

$$AgO\,NO^5 : NH^4\,Cl = 100 : 31{\cdot}488.$$

The preparation of pure lead was attended with greater difficulties even than that of pure silver. It was effected by digesting solution of acetate of lead in a leaden vessel with thin lead-foil until all copper and silver were precipitated. Sulphuric acid was then added so as to form sulphate of lead, which was

converted into carbonate by digestion with ammonia and carbonate of ammonia. This carbonate of lead was dried and reduced by cyanide of potassium. Pure lead was also obtained by the reduction of the chloride, either by fusion with cyanide of potassium, or with a mixture of cyanide and black flux.

The lead was converted into nitrate by heating it with strong nitric acid until it was completely converted into nitrate, and then evaporating to dryness in the same vessel in a current of dry air at 140°.

100 parts of lead gave 159·974 of nitrate of lead.

The synthesis of sulphate of lead gave the relation

$$Pb : PbO\,SO^3 = 100 : 146{\cdot}4275.$$

The analysis of chlorate of potash, that is, the determination of the quantity of oxygen, was effected either by heating chlorate of potash alone, or by heating it with hydrochloric acid. The first of these methods gave the relation

$$KO\,ClO^5 : KCl = 100 : 60{\cdot}8428;$$

and the second

$$KO\,ClO^5 : KCl = 100 : 60{\cdot}849.$$

The analysis of sulphate of silver was effected by reduction with hydrogen.

100 pure sulphate gave 69·203 silver.

The relations of the atomic weights investigated are seen in the following Table, in which the numbers obtained are compared with those which would be required on the hypothesis of Prout:—

		Prout.	Stas.
Ag :	Cl . . .	=100 : 32·87	=100 : 32·8445
Ag :	S . . .	=100 : 14·814	=100 : 14·852
Ag :	$AgO\,NO^5$.	=100 : 157·404	=100 : 157·473
Ag :	K Cl . .	=100 : 68·981	=100 : 69·103
Ag :	Na Cl . .	=100 : 54·166	=100 : 54·2078
Ag :	NH^4Cl .	=100 : 49·537	=100 : 49·5944
$AgO\,NO^5$:	K Cl . .	=100 : 43·823	=100 : 43·878
$AgO\,NO^5$:	NH^4Cl .	=100 : 31·470	=100 : 31·488
Pb :	$PbO\,NO^5$.	=100 : 159·903	=100 : 159·969
Pb :	$PbO\,SO^3$.	=100 : 146·376	=100 : 146·427
$KO\,ClO^5$:	KCl . .	=100 : 60·816	=100 : 60·846
$AgO\,SO^3$:	Ag . . .	=100 : 69·23	=100 : 69·203

The following are the atomic weights deduced from Stas's experiments as compared with those usually admitted, and taking oxygen at 8:—

	Stas.	
Chloride of potassium . .	74·59	74·5
Silver	107·943	108
Chlorine	35·46	35·5
Potassium	39·13	39
Sodium	23·05	23
Ammonium	18·06	18
Nitrogen	14·041	14
Sulphur	16·037	16
Lead (synthesis of PbO SO^3)	103·453	103·5
Lead (synthesis of PbO NO^5)	103·460	

It will thus be seen that the atomic weights of nitrogen and ammonium differ by 4·02 instead of by 4. Hence if the synthesis of nitrate of silver is correct, the atomic weight of hydrogen cannot be exactly one-eighth that of water. The author proposes to return to the synthesis of water, for his researches lead him to believe that the error will be found there rather than in the synthesis of nitrate of silver.

Stas, who at the commencement of his researches had confidence in the hypothesis of Prout, has been led to the conclusion that it is untenable, as well as the modification introduced by Dumas*. He says, in conclusion, "So long as, in establishing the laws of matter, we are to adhere to experiment, we must consider Prout's law as an entire delusion, and must regard the undecomposable bodies of our globe as distinct beings having no relation to each other. The undoubted analogy of properties observed in certain elements must be sought for in other causes than those derived from the ratio of the weight of their acting masses."

In reference to Stas's investigation, Marignac†, in giving an abstract of it, objects to the conclusion as too absolute. He says "he can only form a clear idea of the degree of confidence which the determination of an atomic weight deserves when this weight has been obtained by several methods absolutely independent of each other, based on the analysis of several distinct compounds."

When M. Stas, as a control of the synthesis of nitrate of silver, refers to the experiments by which he determined the proportion between this nitrate and the chloride of potassium, which latter is directly connected with silver, M. Marignac only sees a confirmation of the exactitude of the experiments themselves, but by no means a control of the method.

* Phil. Mag. vol. xvi. p. 209.

† *Bibliothèque Universelle*, vol. ix. p. 202. *Répertoire de Chimie*, May 1861.

If, for instance, we suppose that nitrate of silver does not contain its elements exactly in the proportion of their atomic weights, even the best methods for its analysis or synthesis will give with the same inaccuracy the relation of this weight.

In developing his ideas on this point, M. Marignac refers to his own experiments on monohydrated sulphuric acid. He has shown that this compound, which was always considered very stable, is really very unstable; it is only when it contains a slight excess of water (1 per cent.) that it is quite stable, otherwise the least increase of temperature causes it to give off vapours of anhydrous sulphuric acid.

Who could say *à priori* that the sulphuret and nitrate of silver are not capable of retaining at high temperatures a trace of sulphur or nitric acid, seeing that sulphuric acid can retain a slight excess of water far above 100°?

Causes of error of this kind, to which others might be added, lead M. Marignac to doubt whether the differences between experiment and Prout's law do not arise from the imperfection of experimental methods.

M. Marignac has another objection against Stas's conclusion. "If the numbers of M. Stas do not absolutely coincide with those of Prout, they approximate to such an extent that they in fact cannot be considered accidental. What has been said of Mariotte's and Gay-Lussac's laws may be applied to Prout's laws. These laws, long considered absolute, were found to be inexact when the experiments were made with the accuracy attained by M. Regnault and by M. Magnus. Nevertheless they will always be considered as expressing natural laws, whether from a practical point of view (for by their means the changes of volume in gases may be calculated with sufficient accuracy) or from the point of view of theory; for they probably express the normal law of these changes of volume abstracted from some disturbing influences, the effects of which may some day be calculated. The same may be believed of Prout's law."

M. Marignac terminates his remarks by an observation due to Dumas:—The fundamental principle which led Prout to propound his law, that is to say, the idea of the unity of matter and all the conceptions which have been based on this principle, is quite independent of the magnitude of the unit which might serve as common divisor of the atomic weights. Whether this weight be that of an atom of hydrogen, of half or a quarter of an atom, or whether it be any infinitely small fraction, all these considerations would nevertheless retain the same degree of probability. The relations between the constitution merely become somewhat less simple between the different elements.

XX. *Remark on the Tactic of* 9 *Elements. By* J. J. Sylvester, *M.A., F.R.S., Professor of Mathematics at the Royal Military Academy, Woolwich*.*

AT the end of my preceding paper in this Magazine for July, I hazarded an opinion that any grouping of 28 synthemes comprising the 84 triads belonging to a system of 9 elements, might be regarded as made up of 1 syntheme of monomial triads, 18 synthemes of binomial triads, and 9 of trinomial triads, the denominations (monomial, binomial, and trinomial) having reference to a duly chosen distribution of the 9 elements into 3 nomes of 3 elements each. This conjecture is capable of being brought to a very significant, although not decisive test, by examining a peculiar and important distribution of the 28 synthemes into 7 sets of 4 synthemes each, the property of each set being that its 12 triads contain amongst them all the 36 *duads* appertaining to the 9 elements. I discovered this mode of distribution very many years ago; but it was first published independently by a mathematician whose name I forget, either in the Philosophical Magazine or in the Cambridge and Dublin Mathematical Journal, I think at some time between the years 1847–53. A similar mode of distribution exists for any system of elements of which the number is a power of 3. Without pausing to give the law of formation, I shall simply observe that for 9 elements we may take as a basic arrangement the square

1	2	3
4	5	6
7	8	9

and form from this, by a symmetrical method, the annexed six derived arrangements:—

7 1 2	7 2 3	9 2 3
3 5 6	1 4 5	1 5 6
4 8 9	6 8 9	4 7 8
4 3 1	5 2 3	4 2 3
7 5 6	7 6 4	8 5 6
2 8 9	1 8 9	1 9 7

and reading off each of these squares in lines, in columns, and in right and left diagonal fashion, we obtain the 7 sets of 4 synthemes each referred to, viz.

123	456	789	
147	258	369	
159	267	348	
168	249	357	

* Communicated by the Author.

712 356 489	723 145 689	923 156 478
734 158 269	716 248 359	914 257 368
759 164 238	749 256 318	988 264 317
768 139 254	758 219 346	967 218 354
431 756 289	523 764 189	423 859 197
472 358 169	571 268 349	481 259 367
459 362 178	569 241 378	457 261 389
468 379 152	548 279 361	469 287 351

If, now, we take any distribution of the 9 elements into nomes other than 123, 456, 789, we shall find that some of the synthemes will contain trinomials, some binomials only, but others (in number either 9 or 18, according to the distribution chosen) will contain binomials and trinomials mixed; but if we adopt 123, 456, 789 as the nomes, then it will be found that the remaining 27 synthemes (after excluding the monomial syntheme 123, 456, 789) will consist of 18 purely binomial triads, and 9 purely trinomial triads. The former will consist of the first, second, and fourth synthemes of the 6 derived groups; the latter of the second, third, and fourth of the basic group, and of the second synthemes of each of the 6 derived groups.

It may be remembered that there are two types or species of groupings of trinomial triad synthemes appertaining to 3 nomes of 3 elements; one of these species contains 4, the other 36 individual groupings. It may easily be ascertained that the grouping above indicated belongs to the first (the less numerous) of these species. Again, there are 3 types or species of groupings of binomial triad synthemes appertaining to the same system of nomes; one containing 12, one 24, and the third 108 groupings. The grouping with which we are here concerned will be found to belong to the *second* of these species,—that denoted by the symbols $\left|\begin{smallmatrix}\alpha\\ \epsilon\end{smallmatrix}\right|$ in my paper of last month. Hence, then, we derive a very considerable presumption in favour of the opinion which I advanced at the close of my preceding paper on Tactic, and derived, too, from a case apparently unfavourable to the verisimilitude of the conjecture; for a natural subdivision of 28 things into 7 sets of 4 each seems at first sight hardly compatible with another natural division into 3 sets of 1, 18, and 9 respectively. Notwithstanding this seeming incompatibility, we have found that the two methods of decomposition do coexist, owing essentially to the fact that the 7 sets (of 4 synthemes each) stand not in a relation of

indifference set to set, but are to be considered as composed of a base and 6 derivatives indifferently related to the base and to each other. The theory of these 7 sets is extremely curious, and well worthy of being fully investigated by the student of tactic, but cannot be gone into within the limits suitable to the pages of a philosophical miscellany.

Before taking final leave of the subject (at all events for the present, and in the pages of this Magazine), as I have been questioned as to the meaning of the important word "syntheme," derived from *συν θημα*, I repeat that a "syntheme" is the general name for any consociation of the single or combined elements of a given system of elements in which each element is once and once only contained. A *nome*, from *νεμω* (*to divide*), means a consociation of a certain number out of a given system of elements; and a binomial, trinomial, or r-nomial combination of any specified sort, means a combination whose elements are dispersed between 2, 3, or r of the nomes between which the entire system of elements is supposed to have been divided.

K, Woolwich Common,
July 14, 1861.

P.S. I have found the date and place of the resolution into 7 sets referred to in the text; it is given in a paper by Mr. Kirkman, vol. v. p. 261 of the Cambridge and Dublin Mathematical Journal for 1850. His 7 squares, whose horizontal, vertical, and two diagonal readings (like mine) constitute the 7 sets in question, are substantially as follows:—

	1 2 3 4 5 6 7 8 9	
1 2 4 5 6 7 8 9 3	4 5 7 8 9 1 2 3 6	7 8 1 2 3 4 5 6 9
7 1 2 3 4 5 6 8 9	1 4 5 6 7 8 9 2 3	4 7 8 9 1 2 3 5 6*

On assuming 123, 456, 789 as the three nomes, the 28 syn-

* By changing the positions of the lines and columns of the six derivative squares, which may be done without affecting the value of their *readings*, they may be represented under the form following, which will be seen to render much clearer their relation to the primitive square:—

412 623 423 756 745 956 389 189 178	239 129 127 451 563 453 786 784 896

themes contained in the sets will be found to consist of purely monomial, binomial, and trinomial synthemes.

Thus there would be an additional presumption in favour of the supposed law of *homonomial resolubility*, provided that Mr. Kirkman's solution were essentially distinct in type from my own; his binomial and trinomial systems, taken separately, coincide in type with those afforded by my solution, notwithstanding which it would not be lawful to assume (indeed I had at first some reasons for doubting) the identity of type of the total groupings of which these systems form part; all we could have positively inferred from that fact would have been, that these two groupings both belong to the same class or genus containing 26,880 individuals, the second of the six referred to at the close of my last paper; a comparison of the two solutions has, however, satisfied me that they are absolutely identical in form.

XXI. *Proceedings of Learned Societies.*

ROYAL INSTITUTION OF GREAT BRITAIN.

June 7, 1861.

"ON the Physical Basis of Solar Chemistry." By John Tyndall, Esq., F.R.S., Professor of Natural Philosophy, Royal Institution.

Omitting all preface, the speaker drew attention to an experimental arrangement intended to prove that gaseous bodies radiate heat in different degrees. Behind a double screen of polished tin was placed an ordinary ring gas-burner; on this was placed a hot copper ball, from which a column of heated air ascended: behind the screen, but so placed that no ray from the ball could reach the instrument, was an excellent thermo-electric pile, connected by wires with a very delicate galvanometer. The thermo-electric pile was known to be an instrument whereby heat was applied to the generation of electric currents; the strength of the current being an accurate measure of the quantity of the heat. As long as both faces of the pile were at the same temperature no current was produced; but the slightest difference in the temperature of the two faces at once declared itself by the production of a current, which, when carried through the galvanometer, indicated by the deflection of the needle both its strength and its direction.

The two faces of the pile were in the first instance brought to the same temperature, the equilibrium being shown by the needle of the galvanometer standing at zero. The rays emitted by the current of hot air already referred to were permitted to fall upon one of the faces of the pile; and an extremely slight movement of the needle showed that the radiation from the hot air, though sensible, was extremely feeble. Connected with the ring-burner was a holder containing oxygen gas; and by turning a cock, a stream of this gas was

permitted to issue from the burner, strike the copper ball, and ascend in a heated column in front of the pile. The result was that oxygen showed itself, as a radiator of heat, to be quite as feeble as atmospheric air.

A second holder containing olefiant gas was also connected by its own system of tubes with the ring-burner. Oxygen had already flowed over the ball and cooled it in some degree. Hence, as a radiator in comparison with oxygen, the olefiant gas laboured under a disadvantage. It was purposely arranged that this should be the case; so that if, notwithstanding its being less hot, the olefiant gas showed itself a better radiator, its claim to superiority in this respect would be decisively proved. On permitting the gas to issue upwards, it cast an amount of heat against the adjacent face of the pile sufficient to impel the needle of the galvanometer almost to its stops at 90°. This experiment proved the vast difference between two equally transparent gases with regard to their power of emitting radiant heat.

The converse experiment was now performed. The thermo-electric pile was removed and placed between two cubes filled with water kept in a state of constant ebullition; and it was so arranged that the quantities of heat falling from the cubes on the opposite faces of the pile were exactly equal, thus neutralizing each other. The needle of the galvanometer being at zero, a sheet of oxygen gas was caused to issue from a slit between one of the cubes and the adjacent face of the pile. If this sheet of gas possessed any sensible power of intercepting the thermal rays from the cube, one face of the pile being deprived of the heat thus intercepted, a difference of temperature between its two faces would instantly set in, and the result would be declared by the galvanometer. The quantity absorbed by the oxygen under those circumstances was too feeble to affect the galvanometer; the gas, in fact, proved sensibly transparent to the rays of heat. It had but a feeble power of radiation; it had an equally feeble power of absorption.

The pile remaining in its position, a sheet of olefiant gas was caused to issue from the same slit as that through which the oxygen had passed. No one present could see the gas; it was quite invisible, the light went through it as freely as through oxygen or air, but its effect upon the thermal rays emanating from the cube was what might be expected from a sheet of metal. A quantity so large was cut off that the needle of the galvanometer, promptly quitting the zero line, moved with energy to its stops. Thus the olefiant gas, so light and clear and pervious to luminous rays, was a most potent destroyer of the rays emanating from an obscure source. The reciprocity of action established in the case of oxygen comes out here; the good radiator is found by this experiment to be the good absorber.

This result, which was exhibited before a public audience this evening for the first time, was typical of what had been obtained with gases generally. Going through the entire list of gases and vapours in this way, we should find radiation and absorption to be as rigidly associated as positive and negative in electricity, or as north and south

polarity in magnetism. The gas which, when heated, is most competent to generate a calorific ray, is precisely that which is most competent to stop such a ray. If the radiation be high, the absorption is high; if the radiation be moderate, the absorption is moderate; if the radiation be low, the absorption is low; so that if we make the number which expresses the absorptive power the numerator of a fraction, and that which expresses its radiative power the denominator, the result would be that, on account of the numerator and denominator varying in the same proportion, the value of that fraction would always remain the same, whatever might be the gas or vapour experimented with.

But why should this reciprocity exist? What is the meaning of absorption? what is the meaning of radiation? When you cast a stone into still water, rings of waves surround the place where it falls; motion is radiated on all sides from the centre of disturbance. When the hammer strikes a bell, the latter vibrates; and sound, which is nothing more than an undulatory motion of the air, is radiated in all directions. Modern philosophy reduces light and heat to the same mechanical category. A luminous body is one with its particles in a state of vibration; a hot body is one with its particles also vibrating, but at a rate which is incompetent to excite the sense of vision; and as a sounding body has the air around it, through which it propagates its vibrations, so also the luminous or heated body has a medium called æther, which accepts its motions and carries them forward with inconceivable velocity. Radiation, then, as regards both light and heat, is *the transference of motion from the vibrating body to the æther in which it swings*; and, as in the case of sound, the motion imparted to the air is soon transferred to the surrounding objects, against which the aërial undulations strike, the sound being, in technical language, *absorbed*, so also with regard to light and heat, absorption consists in *the transference of motion from the agitated æther to the particles of the absorbing body*.

The simple atoms are found to be bad radiators; the compound atoms good ones: and the higher the degree of complexity in the atomic grouping, the more potent, as a general rule, is the radiation and absorption. Let us get definite ideas here, however gross, and purify them afterwards by the process of abstraction. Imagine our simple atoms swinging like single spheres in the æther; they cannot create the swell which a group of them united to form a system can produce. An oar runs freely edgeways through the water, and imparts far less of its motion to the water than when its broad flat side is brought to bear upon it. In our present language the oar, broad side vertical, is a good radiator; broad side horizontal, it is a bad radiator. Conversely, the waves of water, impinging upon the flat face of the oar-blade, will impart a greater amount of motion to it than when impinging upon the edge. In the position in which the oar radiates well it also absorbs well. Simple atoms glide through the æther without much resistance; compound ones encounter this, and yield up more speedily their motion to the æther. *Mix* oxygen and nitrogen mechanically, they absorb and radiate a certain amount.

Cause these gases to *combine* chemically and form nitrous oxide, both the absorption and radiation are thereby augmented 250 times!

In this way we look with the telescope of the intellect into atomic systems, and obtain a conception of processes which the eye of sense can never reach. But gases and vapours possess a power of choice as to the rays which they absorb. They single out certain groups of rays for destruction, and allow other groups to pass unharmed. This is best illustrated by a famous experiment of Sir David Brewster's, modified to suit the requirements of the present discourse. Into a glass cylinder, with its ends stopped by discs of plate glass, a small quantity of nitrous acid gas was introduced, the presence of the gas being indicated by its rich brown colour. The beam from an electric lamp being sent through two prisms of bisulphide of carbon, a spectrum 7 feet long and 18 inches wide was cast upon a screen. Introducing the cylinder containing the nitrous acid into the path of the beam as it issued from the lamp, the splendid and continuous spectrum became instantly furrowed by numerous dark bands, the rays answering to which were struck down by the nitric gas, while it permitted the light which fell upon the intervening spaces to pass with comparative impunity.

Here also the principle of reciprocity, as regards radiation and absorption, holds good; and could we, without otherwise altering its physical character, render that nitrous gas luminous, we should find that the very rays which it absorbs are precisely those which it would emit. When atmospheric air and other gases are brought to a state of intense incandescence by the passage of an electric spark, the spectra which we obtain from them consist of a series of bright bands. But such spectra are produced with the greatest brilliancy when, instead of ordinary gases, we make use of metals heated so highly as to volatilize them. This is easily done by the voltaic current. A capsule of carbon was filled with mercury, which formed the positive electrode of the electric lamp; a carbon-point was brought down upon this; and on separating one from the other, a brilliant arc containing the mercury in a volatilized condition passed between them. The spectrum of this arc was not continuous like that from the solid carbon points, but consisted of a series of vivid bands, each corresponding in colour to that particular portion of the spectrum to which its rays belonged. Copper gave its system of bands; zinc gave its system; and brass, which is an alloy of copper and zinc, gave a splendid spectrum made up of the bands belonging to both metals.

Not only, however, when metals are united like zinc and copper to form an alloy is it possible to obtain the bands which belonged to them. No matter how we may disguise the metal—allowing it to unite with oxygen to form an oxide, and this again with an acid to form a salt; if the heat applied be sufficiently intense, the bands belonging to the metal reveal themselves with perfect definition. Holes were drilled in a cylinder of retort carbon, and, these being filled with pure culinary salt, the carbon was made the positive electrode of the lamp; the resultant spectrum showed the brilliant yellow lines of the metal sodium. Similar experiments were made with the

chlorides of strontium, calcium, lithium*, and other metals; each salt gave the bands due to the metal. Different salts were then mixed together and rammed into the holes in the carbon; a spectrum was obtained which contained the bands of them all.

The position of these bright bands never varies; and each metal has its own system. Hence the competent observer can infer from the bands of the spectrum the metals which produce it. It is a language addressed to the eye instead of the ear; and the certainty would not be augmented if each metal possessed the power of audibly calling out, "I am here!" Nor is this language affected by distance. If we find that the sun or the stars give us the bands of our terrestrial metals, it is a declaration on the part of these orbs that such metals enter into their composition. Does the sun give us any such intimation? Does the solar spectrum exhibit bright lines which we might compare with those produced by our terrestrial metals, and prove either their identity or difference? No. The solar spectrum, when closely examined, gives us a multitude of fine dark lines instead of bright ones. They were first noticed by Dr. Wollaston, were investigated with profound skill by Fraunhofer, and named from him Fraunhofer's lines. They have been long a standing puzzle to philosophers. The bright lines which the metals give us have been also known to us for years; but the connexion between both classes of phenomena was wholly unknown, until Kirchhoff, with admirable acuteness, revealed the secret, and placed it at the same time in our power to chemically analyse the sun.

We have now some hard work before us; hitherto we have been delighted by objects which addressed themselves rather to our æsthetic taste than to our scientific faculty. We have ridden pleasantly to the base of the final cone of Etna, and must now dismount and march wearily through ashes and lava, if we would enjoy the prospect from the summit. Our problem is to connect the dark lines of Fraunhofer with the bright ones of the metals. The white beam of the lamp is refracted in passing through our two prisms, but its different components are refracted in different degrees, and thus its colours are drawn apart. Now the colour depends solely upon the rate of oscillation of the particles of the luminous body,—red light being produced by one rate, blue light by a much quicker rate, and the colours between red and blue by the intermediate rates. The solid incandescent coal-points give us a continuous spectrum; or, in other words, they emit rays of all possible periods between the two extremes of the spectrum. They have particles oscillating so as to produce red; others to produce orange; others to produce yellow, green, blue, indigo, and violet respectively. Colour, as many of you know, is to light what *pitch* is to sound. When a violin-player

* The vividness of the colours of the lithium spectrum is extraordinary: it contained a blue band of indescribable splendour. It was thought by many, during the discourse, that I had mistaken strontium for lithium, as this blue band had never before been seen. I have obtained it many times since; and my friend Dr. Miller, having kindly analysed the substance made use of, pronounces it chloride of lithium.—J. T.

presses his finger on a string he makes it shorter and tighter, and thus, causing it to vibrate more speedily, augments the pitch. Imagine such a player to move his finger slowly along the string, shortening it gradually as he draws his bow, the note would rise in pitch by a regular gradation; there would be no gap intervening between note and note. Here we have the analogue to the continuous spectrum, whose colours insensibly blend together without gap or interruption, from the red of the lowest pitch to the violet of the highest. But suppose the player, instead of gradually shortening his string, to press his finger on a certain point, and to sound the corresponding note; then to pass on to another point more or less distant, and sound its note; then to another, and so on, thus sounding particular notes separated from each other by gaps which correspond to the intervals of the string passed over; we should then have the exact analogue of a spectrum composed of separate bright bands with intervals of darkness between them. But this, though a perfectly true and intelligible analogy, is not sufficient for our purpose; we must look with the mind's eye at the very oscillating atoms of the volatilized metal. Figure these atoms connected by springs of a certain tension, and which, if the atoms are squeezed together, push them asunder, or, if the atoms are drawn apart, pull them together, causing them, before coming to rest, to quiver at a certain definite rate determined by the strength of the spring. Now the volatilized metal which gives us one bright band is to be figured as having its atoms united by springs all of the same tension, its vibrations are all of one kind. The metal which gives us two bands may be figured as having some of its atoms united by springs of one tension, and others by a second series of springs of a different tension. Its vibrations are of two distinct kinds; so also when we have three or more bands, we are to figure as many distinct sets of springs, each set capable of vibrating in its own particular time and at a different rate from the other. If we seize this idea definitely, we shall have no difficulty in dropping the metaphor of springs, and substituting for it mentally the forces by which the atoms act upon each other. Having thus far cleared our way, let us make another effort to advance.

Here is a pendulum,—a heavy ivory ball suspended from a string. I blow against this ball; a single puff of my breath moves it a little way from its position of rest; it swings back towards me, and when it reaches the limit of its swing I puff again. It now swings further; and thus by timing my puffs I can so accumulate their action as to produce oscillations of large amplitude. The ivory ball here has absorbed the motions which my breath communicated to the air. I now bring the ball to rest. Suppose, instead of my breath, a wave of air to strike against it, and that this wave is followed by a series of others which succeed each other exactly in the same intervals as my puffs; it is perfectly manifest that these waves would communicate their motion to the ball and cause it to swing as the puffs did. And it is equally manifest that this would not be the case if the impulses of the waves were not properly timed; for then the motion

imparted to the pendulum by one wave would be neutralized by another, and there could not be that accumulation of effect which we have when the periods of the waves correspond with the periods of the pendulum. So much for the kind of impulses absorbed by the pendulum. But such a pendulum set oscillating in air produces waves in the air; and we see that the waves which it produces must be of the same period as those whose motions it would take up or absorb most copiously if they struck against it. Just in passing I may remark that, if the periods of the waves be double, treble, quadruple, &c. the periods of the pendulum, the shocks imparted to the latter would also be so timed as to produce an accumulation of motion.

Perhaps the most curious effect of these timed impulses ever described was that observed by a watchmaker named Ellicott, in the year 1741. He set two clocks leaning against the same rail; one of them, which we may call A, was set going; the other, B, not. Some time afterwards he found, to his surprise, that B was ticking also. The pendulums being of the same length, the shocks imparted by the ticking of A to the rail against which both clocks rested were propagated to B, and were so timed as to set B going. Other curious effects were at the same time observed. When the pendulums differed from each other a certain amount, A set B going, but the reaction of B stopped A. Then B set A going, and the reaction of A stopped B. If the periods of oscillation were close to each other, but still not quite alike, the clocks mutually controlled each other, and by a kind of mutual compromise they ticked in perfect unison.

But what has all this to do with our present subject? They are mechanically identical. The varied actions of the universe are all modes of motion; and the vibration of a ray claims strict brotherhood with the vibrations of our pendulum. Suppose æthereal waves striking upon atoms which oscillate in the same periods as the waves succeed each other, the motion of the waves will be absorbed by the atoms; suppose we send our beam of white light through a sodium flame, the particles of that flame will be chiefly affected by those undulations which are synchronous with their own periods of vibration. There will be on the part of those particular rays a transference of motion from the agitated æther to the atoms of the volatilized sodium, which, as already defined, is absorption. We use glass screens to defend us from the heat of our fires; how do they act? Thus:—The heat emanating from the fire is for the most part due to radiations which are incompetent to excite the sense of vision; we call these rays obscure. Glass, though pervious to the luminous rays, is opake in a high degree to those obscure rays, and cuts them off, while the cheerful light of the fire is allowed to pass. Now mark me clearly. The heat cut off from your person is to be found in the glass, the latter becomes heated and radiates towards your person; what, then, is the use of the glass if it merely thus acts as a temporary halting-place for the rays, and sends them on afterwards? It does this:—it not only sends the heat it receives towards you, but

scatters it also in all other directions round the room. Thus the rays which, were the glass not interposed, would be shot directly against your person, are for the most part diverted from their original direction, and you are preserved from their impact.

Now for our experiment. I pass the beam from the electric lamp through the two prisms, and the spectrum spreads its colours upon the screen. Between the lamp and the prism I interpose this snap-dragon light. Alcohol and water are here mixed up with a quantity of common salt, and the metal dish that contains them is heated by a spirit-lamp. The vapour from the mixture ignites, and we have this monochromatic flame. Through this flame the beam from the lamp is now passing; and observe the result upon the spectrum. You see a dark band cut out of the yellow,—not very dark, but sufficiently so to be seen by everybody present. Observe how the band quivers and varies in shade as the amount of yellow light cut off by the unsteady flame varies in amount. The flame of this monochromatic lamp is at the present moment casting its proper yellow light upon that shaded line; and more than this, it casts in part the light which it absorbs from the electric lamp upon it; but it scatters the greater portion of this light in other directions, and thus withdraws it from its place upon the screen; as the glass, in the case above supposed, diverted the heat of the fire from your person. Hence the band appears dark; not absolutely, but dark in comparison with the adjacent brilliant portions of the spectrum.

But let me exalt this effect. I place in front of the electric lamp the intense flame of a large Bunsen's burner. I have here a platinum capsule into which I put a bit of sodium less than a pea in magnitude. The sodium placed in the flame soon volatilizes and burns with brilliant incandescence. Observe the spectrum. The yellow band is clearly and sharply cut out, and a band of intense obscurity occupies its place. I withdraw the sodium, the brilliant yellow of the spectrum takes its proper place: I reintroduce the sodium, and the black band appears.

Let me be more precise:—The yellow colour of the spectrum extends over a sensible space, blending on one side into orange and on the other into green. The term "yellow band" is therefore somewhat indefinite. I want to show you that it is the precise yellow band emitted by the volatilized sodium which the same substance absorbs. By dipping the coal-point used for the positive electrode into a solution of common salt, and replacing it in the lamp, I obtain that bright yellow band which you now see drawn across the spectrum. Observe the fate of that band when I interpose my sodium light. It is first obliterated, and instantly that black streak occupies its place. See how it alternately flashes and vanishes as I withdraw and introduce the sodium flame!

And supposing that instead of the flame of sodium alone I introduce into the path of the beam a flame in which lithium, strontium, magnesium, calcium, &c. are in a state of volatilization, each metallic vapour would cut out its own system of bands, each corresponding exactly in position with the bright band which that metal itself would

cast upon the screen. The light of our electric lamp then shining through such a composite flame would give us a spectrum cut up by dark lines, exactly as the solar spectrum is cut up by the lines of Fraunhofer.

And hence we infer the constitution of the great centre of our system. The sun consists of a nucleus which is surrounded by a flaming atmosphere. The light of the nucleus would give us a continuous spectrum, as our common coal-points did; but having to pass through the photosphere, as our beam through the flame, those rays of the nucleus which the photosphere can itself emit are absorbed, and shaded spaces, corresponding to the particular rays absorbed, occur in the spectrum. Abolish the solar nucleus, and we should have a spectrum showing a bright band in the place of every dark line of Fraunhofer. These lines are therefore not absolutely dark, but dark by an amount corresponding to the difference between the light of the nucleus intercepted by the photosphere, and the light which issues from the latter.

The man to whom we owe this beautiful generalization is Kirchhoff, Professor of Natural Philosophy in the University of Heidelberg; but, like every other great discovery, it is compounded of various elements. Mr. Talbot observed the bright lines in the spectra of coloured flames. Sixteen years ago Dr. Miller gave drawings and descriptions of the spectra of various coloured flames. Wheatstone, with his accustomed ingenuity, analysed the light of the electric spark, and showed that the metals between which the spark passed determined the bright bands in the spectrum of the spark. Masson published a prize essay on these bands; Van der Willigen, and more recently Plücker, have given us beautiful drawings of the spectra obtained from the discharge of Ruhmkorff's coil. But none of these distinguished men betrayed the least knowledge of the connexion between the bright bands of the metals and the dark lines of the solar spectrum. The man who came nearest to the philosophy of the subject was Angström. In a paper translated from Poggendorff's *Annalen* by myself, and published in the Philosophical Magazine for 1855, he indicates that the rays which a body absorbs are precisely those which it can emit when rendered luminous. In another place he speaks of one of his spectra giving the general impression of *reversal* of the solar spectrum. Foucault, Stokes, and Thomson have all been very close to the discovery; and, for my own part, the examination of the radiation and absorption of heat by gases and vapours, some of the results of which I placed before you at the commencement of this discourse, would have led me in 1859 to the law on which all Kirchhoff's speculations are founded, had not an accident withdrawn me from the investigation. But Kirchhoff's claims are unaffected by these circumstances. True, much that I have referred to formed the necessary basis of his discovery; so did the laws of Kepler furnish to Newton the basis of the theory of gravitation. But what Kirchhoff has done carries us far beyond all that had before been accomplished. He has introduced the order of law amid a vast assemblage of empi-

rical observations, and has ennobled our previous knowledge by showing its relationship to some of the most sublime of natural phenomena.

Postscript, July 24.—As far back as the year 1822 Sir John Herschel described the spectra of various coloured flames in the Transactions of the Royal Society of Edinburgh. In his "Treatise on Light" in the *Encycl. Metropol.*, published in 1827, he describes the spectra derived from the introduction of various salts into flames, and finds exactly as Bunsen and Kirchhoff have recently found, the muriates most suitable for such experiments, on account of their volatility. He also adds the distinct statement, that "the colours thus communicated by different bases to flame afford in many cases a ready and neat way *of detecting extremely minute quantities of them.*"

ROYAL SOCIETY.

[Continued from p. 77.]

November 22, 1860.—Major-General Sabine, R.A., Treasurer and Vice-President, in the Chair.

The following communications were read :—

"Contributions towards the History of the Monamines."—No. III. Compound Ammonias by Inverse Substitution. By A. W. Hofmann, LL.D., F.R.S. &c. Received July 24, 1860.

Many years ago I showed that the bromide or iodide of a quartary ammonium splits under the influence of heat into the bromide or iodide of an alcohol-radical on the one hand, and a tertiary monamine on the other.

Having lately returned to the study of this class of substances, I was led to examine the deportment, under the influence of heat, of the tertiary, secondary, and, lastly, of the primary monammonium-salts.

Experiment has shown that these substances undergo an analogous decomposition. The chloride of a tertiary monammonium when submitted to distillation yields, together with the chloride of an alcohol-radical, a secondary monamine; the chloride of a secondary monammonium, together with an alcohol-chloride, a primary monamine; lastly, the chloride of a primary monammonium, the chloride of an alcohol-radical and ammonia.

Exactly, then, as my former experiments show that we may rise in the scale by replacing the four equivalents of hydrogen in ammonium one by one by radicals, so it is obvious from these new experiments that we may also step by step descend, by substituting again hydrogen for the radicals in succession.

To take as an illustration the monammonium-salts of the ethyle-series which as yet I have chiefly examined :

Ascent.

$$\underbrace{H_3N}_{\text{Ammonia.}} + \underbrace{(C_2H_5)Br}_{\text{Bromide of ethyle.}} = \underbrace{[(C_2H_5)H_3N]Br}_{\text{Bromide of Ethylammonium.}}$$

$$\underbrace{(C_2H_5)H_2N}_{\text{Ethylamine.}} + (C_2H_5)Br = \underbrace{[(C_2H_5)_2H_2N]Br}_{\text{Bromide of Diethylammonium.}}$$

$$\underbrace{(C_2H_5)_2HN}_{\text{Diethylamine.}} + (C_2H_5)Br = \underbrace{[(C_2H_5)_3HN]Br}_{\text{Bromide of Triethylammonium.}}$$

$$\underbrace{(C_2H_5)_3N}_{\text{Triethylamine.}} + (C_2H_5)Br = \underbrace{[(C_2H_5)_4N]Br}_{\text{Bromide of Tetrethylammonium.}}$$

Note.—H=1; C=12.

Descent.

$$\underbrace{[(C_2H_5)_4N]Cl}_{\text{Chloride of Tetrethylammonium.}} = \underbrace{(C_2H_5)Cl}_{\text{Chloride of Ethyle.}} + \underbrace{(C_2H_5)_3N}_{\text{Triethylamine.}}$$

$$\underbrace{[(C_2H_5)_3HN]Cl}_{\text{Chloride of Triethylammonium.}} = (C_2H_5)Cl + \underbrace{(C_2H_5)_2HN}_{\text{Diethylamine.}}$$

$$\underbrace{[(C_2H_5)_2H_2N]Cl}_{\text{Chloride of Diethylammonium.}} = (C_2H_5)Cl + \underbrace{(C_2H_5)H_2N}_{\text{Ethylamine.}}$$

$$\underbrace{[(C_2H_5)H_3N]Cl}_{\text{Chloride of Ethylammonium.}} = (C_2H_5)Cl + \underbrace{H_3N}_{\text{Ammonia.}}$$

The above reactions, interesting when regarded from a scientific point of view, admit of but limited application in practice. The purity of the result is disturbed by several circumstances, which it is difficult to exclude. Unless the temperature be sufficiently high, a small portion of the ammonium-salt submitted to distillation sublimes without change; again, a portion of the same salt is reproduced in the neck of the retort and in the receiver*, from the very constituents into which it splits; lastly, if the temperature be too high, the chloride of ethyle is apt to be decomposed into ethylene and hydrochloric acid, the latter producing, with the monamine liberated in the reaction, a salt which in its turn is likewise decomposed.

Thus the chloride of diethylammonium, for instance, together with chloride of ethyle and ethylamine, yields ethylene and chloride of ethylammonium which splits into chloride of ethyle and ammonia.

The idea naturally suggested itself, to attempt, by means of this

* This inconvenience may be partly obviated by distilling into an acid.

reaction, the formation of the primary and secondary monophosphines, which are at present unknown. Experiments made with the view of transforming triethylphosphine into diethylphosphine have as yet remained unsuccessful, the chloride of triethylphosphonium distilling without alteration.

"Notes of Researches on the Poly-Ammonias."—No. IX. Remarks on *anomalous* Vapour-densities. By A. W. Hofmann, LL.D., F.R.S. Received July 24, 1860.

In a note addressed to the Royal Society* at the commencement of this year, I have shown that the molecules of the diamines, like those of all other well-examined compounds, correspond to two volumes of vapour†, and I have endeavoured to explain the apparent anomalous vapour-densities of the hydrated diamines by assuming that the vapour-volume experimentally obtained was a mixture of the vapour of the anhydrous base and of the vapour of water. Thus, hydrated ethylene-diamine was assumed to split under the influence of heat into anhydrous ethylene-diamine (2 vols. of vapour) and water (2 vols. of vapour).

$$C_2H_{10}N_2O = \left.\begin{matrix}(C_2H_4)''\\ H_2\\ H_2\end{matrix}\right\}N_2 + \left.\begin{matrix}H\\ H\end{matrix}\right\}O\ddagger.$$

The vapour-density of ethylene-diamine referred to hydrogen being 30, and that of water-vapour 9, the vapour-density of a mixture of equal volumes of ethylene-diamine and water-vapour $=\frac{30+9}{2}=19{\cdot}5$, which closely agrees with the result of experiment.

In continuing the study of the diamines, I have expanded these experiments. Without going into the detail of the inquiry, I beg leave to record an observation which appears to furnish an experimental solution to the question.

Ethylene-diamine, when submitted to the action of iodide of ethyle, yields a series of ethylated derivatives, amongst which the diethylated compound has claimed my particular attention. This body in the anhydrous state is an oily liquid containing

$$C_6H_{16}N_2 = \left.\begin{matrix}(C_2H_4)''\\ (C_2H_5)_2\\ H_2\end{matrix}\right\}N_2.$$

With water it forms a beautiful crystalline very stable hydrate§, of the composition

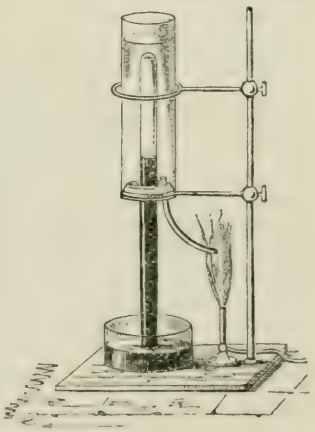
$$C_{10}H_{14}N_2O = \left.\begin{matrix}(C_2H_4)''\\ (C_2H_5)_2\\ H_2\end{matrix}\right\}N_2 + \left.\begin{matrix}H\\ H\end{matrix}\right\}O.$$

* Phil. Mag. vol. xx. p. 66.
† $H_2O=2$ vols. ‡ $H=1$; $O=16$; $C=12$, &c.
§ Phil. Mag. vol. xix. p. 232.

The vapour-density of the anhydrous base was found by experiment to be 57·61, showing that the molecule of diethyl-ethylene-diamine corresponds to 2 vols. of vapour, the theoretical density being $\frac{114}{2}=57$.

On submitting the crystalline hydrate to experiment, I arrived at the vapour-density 33·2. This number is in perfect accordance with the result obtained in the case of ethylene-diamine. The legitimate interpretation of this number is that here again the hydrated base splits into the anhydrous diamine and water, and that the density observed is that of a mixture of equal volumes of diamine-vapour and of water-vapour, the theoretical density of which is $\frac{57+9}{2}=33$. The correctness of this interpretation admits of an elegant experimental demonstration.

Having observed that the hydrate loses its water when repeatedly distilled with a large excess of anhydrous baryta, the idea suggested itself, to attempt the decomposition of the hydrate in the state of *vapour*. If the vapour obtained by heating this hydrate to a temperature 15° or 20° higher than its boiling-point actually consisted of a mixture of equal volumes of its two proximate constituents in a state of *dissociation* (to use a happy term proposed by Deville), it appeared very probable that the volume would be halved by the introduction of anhydrous baryta. Experiment has verified this anticipation.



The upper half of a glass tube filled with, and inverted over, mercury, was surrounded by a second glass tube open at both ends and of a diameter about treble that of the former, the annular space between the two being closed at the bottom of the outer tube by a well-fitting cork. The vessel thus formed round the upper part of the inner tube was moreover provided with a small bent copper tube open at the top and closed at the bottom, which was likewise fixed in the cork. The vessel being filled with paraffin and a lamp being applied to the copper tube, the upper part of the mercury-tube could be conveniently kept at a high and constant temperature, whilst the lower end, immersed in the mercury-trough, remained accessible. A glance at the figure explains the disposition of the apparatus. A small quantity of the hydrated base was then allowed to rise on the top of the mercury in the tube; and the paraffin bath having been heated to 170°, the volume of the vapour was observed. Several pellets of

anhydrous baryta were then allowed to ascend into the vapour-volume, while the temperature was maintained constant. The mercury began immediately to rise, becoming stationary again, when a fraction of the vapour had disappeared, which amounted, the necessary corrections being made, to half the original volume.

"Notes of Researches on the Poly-Ammonias."—No. X. On Sulphamidobenzamine, a new base; and some Remarks upon Ureas and so-called Ureas. By A. W. Hofmann, LL.D., F.R.S. Received July 24, 1860.

Among the numerous compounds capable of the metamorphosis involved in Zinin's beautiful reaction, the nitriles have hitherto escaped the attention of chemists. This is the more remarkable, since some of these bodies are easily converted into crystalline nitro-compounds.

When examining several of the diamines which I have lately submitted to the Royal Society*, I was induced to study the transformation which benzonitrile undergoes under the successive influence of nitric acid and reducing agents.

Benzonitrile, when treated with a mixture of sulphuric and fuming nitric acid, furnishes, as is well known, a solid nitro-substitute which crystallizes from alcohol in beautiful white needles, containing

$$C_7 H_4 N_2 O_2 = C_7 (H_4, NO_2) N\dagger.$$

In order to obtain this body, it is desirable to perform the operation with small quantities, and to cool the liquid carefully, otherwise the formation of appreciable proportions of nitrobenzoic acid can scarcely be avoided.

The nitro-compound is readily attacked by an aqueous solution of sulphide of ammonium; sulphur is abundantly precipitated, and on evaporating the liquid, a yellowish red oil is separated, which gradually and imperfectly solidifies. This substance possesses the characters of a weak base, dissolving with facility in acids, and being again precipitated by the addition of ammonia and the alkalies. The preparation in the state of purity, both of the base itself and of its compounds, presents some difficulty. This circumstance has prevented me from analysing the base. I have, however, examined one of its products of decomposition, which leaves no doubt that nitrobenzonitrile, under the influence of reducing agents, undergoes the well-known transformation of nitro-compounds, and that the composition of the oily base is represented by the formula

$$C_7 H_6 N_2 = C_7 H_4 (NH_2) N.$$

The oily base, when left in contact with sulphide of ammonium, is gradually changed, a crystalline compound being formed, which is easily soluble in alcohol and in ether, but difficultly soluble in water, and which may be purified by several crystallizations from boiling

* Phil. Mag. vol. xix. p. 232. † H=1; O=16; C=12, &c.

water, being deposited on cooling in white brilliant needles. This compound is a well-defined organic base; it dissolves with facility in acids, and is precipitated from these solutions by the addition of potassa or of ammonia. With hydrochloric acid it forms a crystallizable salt, which yields, with dichloride of platinum, an orange-yellow crystalline precipitate.

On analysis, the new base was found to have the composition

$$C_7 H_8 N_2 S,$$

explaining its formation, in which evidently two phases have to be distinguished:

$$(1)\quad C_7(H_4 NO_2) N + 3H_2 S = 2H_2O + 3S + C_7 H_6 N_2,$$
$$(2)\quad C_7 H_6 N_2 + H_2 S = C_7 H_8 N_2 S.$$

The new sulphuretted base has the same composition as sulphocarbonyl-phenyldiamide, a feebly basic compound which I obtained some time ago by the action of ammonia on sulphocyanide of phenyle*.

$$C_7 H_5 NS + H_3 N = C_7 H_8 N_2 S.$$

A superficial comparison of the properties of the two bodies shows, however, that they are only isomeric, the constitution of the latter compound being represented by the expression

$$\left.\begin{matrix}(CS)'' \\ (C_6 H_5)' \\ H_3\end{matrix}\right\} N_2,$$

whilst the constitution of the former may be expressed by the formula

$$\left.\begin{matrix}C_7(H_4 H_2 N) S \\ H \\ H\end{matrix}\right\} N = \left.\begin{matrix}(C_7 H_4 S)'' \\ H_2 \\ H_2\end{matrix}\right\} N_2.$$

The new sulphuretted base is closely connected with an interesting compound which Chancel obtained some years ago, when he submitted nitrobenzamide to the action of reducing agents. The crystalline base produced in this reaction contains

$$C_7 H_8 N_2 O,$$

and differs from the body which forms the subject of this note only by having oxygen in the place of sulphur.

The formation of this oxygenated compound has given rise to some misconceptions, which I take this opportunity to elucidate. A short time before the discovery of the body in question, I had obtained a compound of exactly the same composition by the action of the vapour of cyanic acid upon aniline,

$$C_6 H_7 N + C H N O = C_7 H_8 N_2 O.$$

The mode of producing this substance pointed it out as an ana-

* Phil. Mag. vol. xvii. p. 65.

logue of urea, and hence the designation *aniline-urea*, under which I described the new body as the first of the group of compound ureas, which has since been so remarkably enriched by Wurtz and several other chemists.

The aniline-urea, or phenyl-urea as it is more appropriately called, differs from ordinary urea in its deportment with acids, being, in fact, no longer capable of producing saline compounds. The absence of basic properties in the new phenyl-compound was sufficient to throw some doubt upon its ureic character, and this doubt appeared to receive additional support by Chancel's subsequent discovery of a compound possessing not only the composition of phenyl-urea, but forming likewise well-defined saline combinations. This compound is, however, the amide of amidobenzoic acid, its constitution being interpreted by Chancel, in accordance with its formation :

$$\text{Benzamide} \quad \left.\begin{matrix} C_7H_5O \\ H \\ H \end{matrix}\right\} N.$$

$$\text{Nitrobenzamide} \quad \left.\begin{matrix} C_7(H_4NO_2)O \\ H \\ H \end{matrix}\right\} N.$$

$$\text{Amidobenzamide} \quad \left.\begin{matrix} C_7(H_4NH_2)O \\ H \\ H \end{matrix}\right\} N.$$

Nevertheless chemists, by silent but general consent, began to look upon this compound as the *true* phenyl-urea ; and in most manuals, even Gerhardt's 'Traité de Chimie' not excepted, it figures under this appellation.

Let us see how far this view is supported by the deportment of this substance. Compound ureas, as I conceive the character of this class, must imitate the deportment of urea *par excellence*, both in their mode of formation and their products of decomposition. Urea is formed whenever cyanic acid or cyanates come in contact with ammonia or ammoniacal salts. These are precisely the conditions under which the substance which I have described as phenyl-ureais generated. This compound is obtained by the union of cyanic acid with phenylamine, or of ammonia with cyanate of phenyle.

$$\left.\begin{matrix} C_6H_5 \\ H \\ H \end{matrix}\right\} N + \left.\begin{matrix} (CO)'' \\ H \end{matrix}\right\} N = \left.\begin{matrix} (CO)'' \\ (C_6H_5) \\ H \end{matrix}\right\} N_2.$$

$$\left.\begin{matrix} H \\ H \\ H \end{matrix}\right\} N + \left.\begin{matrix} (CO)'' \\ (C_6H_5) \end{matrix}\right\} N = \left.\begin{matrix} (CO)'' \\ (C_6H_5) \\ H \end{matrix}\right\} N_2.$$

On the other hand, no cyanogen-compound is involved in the formation of amidobenzamide, or amidobenzamine, as it might be more appropriately called, on account of its basic properties.

Not less decisive is the evidence furnished by the products of decomposition of the two bodies. The most characteristic transformation of urea is its decomposition into ammonia and carbonic acid when it is submitted to the action of the alkalies. A compound urea thus treated should yield, together with carbonic acid and ammonia, the monamine from which it has arisen. Phenyl-urea should furnish carbonic acid, ammonia, and phenylamine: these are precisely the products observed in the decomposition of the compound which is formed by the action of cyanic acid on phenylamine.

$$\left.\begin{matrix}(CO)''\\ C_6H_3\\ H_2\end{matrix}\right\}N_2+\left.\begin{matrix}H\\ H\end{matrix}\right\}O=(CO)''O+\left.\begin{matrix}H\\ H\\ H\end{matrix}\right\}N+\left.\begin{matrix}C_6H_5\\ H\\ H\end{matrix}\right\}N.$$

Amidobenzamine, on the other hand, exhibits with potassa the deportment of an amidated amide. The reaction presents two distinct phases, ammonia and amidobenzoic acid being formed in the first phase, and ammonia and benzoic acid in the second:

$$\left.\begin{matrix}C_7(H_4H_2N)O\\ H\\ H\end{matrix}\right\}N+\left.\begin{matrix}H\\ H\end{matrix}\right\}O=\left.\begin{matrix}H\\ H\\ H\end{matrix}\right\}N+\left.\begin{matrix}C_7(H_4H_2N)O\\ H\end{matrix}\right\}O$$

$$\left.\begin{matrix}C_7(H_4H_2N)O\\ H\end{matrix}\right\}O+\left.\begin{matrix}H\\ H\end{matrix}\right\}O=\left.\begin{matrix}H\\ H\\ H\end{matrix}\right\}N+\left.\begin{matrix}C_7H_5O\\ H\end{matrix}\right\}O.$$

No trace of carbonic acid and no trace of phenylamine are eliminated by potassa. It is only by fusing with soda-lime that a perfect destruction of the compound ensues, when, as Chancel has distinctly observed, in the first place ammonia, and ultimately carbonic acid and phenylamine are evolved.

What I have said respecting phenyl-urea applies with equal force to diphenyl-urea. Gerhardt describes as diphenyl-urea the compound obtained by Laurent and Chancel when they examined the action of reducing agents upon nitrobenzophenone, and which, on account of its yellow colour, was originally described as *flavine*. This body contains

$$C_{13}H_{12}N_2O,$$

which is certainly the formula of diphenyl-urea. But here again chemists have been misled by the basic properties of the substance.

It is not my object at present to dwell on the constitution of flavine, which I intend to examine in a subsequent note; suffice it to say that this substance is not diphenyl-urea.

The true diphenyl-urea is the substance commonly called carbanilide, or carbophenylamide.

$$C_{13}H_{12}N_2O=\left.\begin{matrix}(CO)''\\ (C_6H_5)_2\\ H_2\end{matrix}\right\}N_2.$$

Both the conditions under which this body forms, and the pro-

ducts into which it is decomposed, leave no doubt regarding its position in the system.

This compound is formed by the action of cyanate of phenyle upon either water or phenylamine.

$$2\left[\begin{matrix}(CO)''\\(C_6H_5)\end{matrix}\right\}N\Big] + \left.\begin{matrix}H\\H\end{matrix}\right\}O = (CO)''O + \left.\begin{matrix}(CO)''\\(C_6H_5)_2\\H_2\end{matrix}\right\}N_2,$$

$$\left.\begin{matrix}(CO)''\\(C_6H_5)\end{matrix}\right\}N + \left.\begin{matrix}C_6H_5\\H\\H\end{matrix}\right\}N = \left.\begin{matrix}(CO)''\\(C_6H_5)_2\\H_2\end{matrix}\right\}N_2.$$

When boiled with potassa, it splits into carbonic acid and phenylamine.

$$\left.\begin{matrix}(CO)''\\(C_6H_5)_2\\H_2\end{matrix}\right\}N_2 + \left.\begin{matrix}H\\H\end{matrix}\right\}O = (CO)''O + 2\left[\left.\begin{matrix}C_6H_5\\H\\H\end{matrix}\right\}N\right].$$

These are the characters of *true* diphenyl-urea.

GEOLOGICAL SOCIETY.

[Continued from p. 78.]

May 8, 1861.—Leonard Horner, Esq., President, in the Chair.

The following communications were read :—

1. "Description of two Bone-caves in the Mountain of Ker, at Massat, in the Department of the Arriège." By M. Alfred Fontan. Communicated by M. E. Lartet, For. Mem. G.S.

The valley of Massat, on the northern side of the Pyrenees, is of a triangular shape, its northern angle being narrowed by the projecting limestone mountain of Ker. Among the fissures and grottos that traverse this mountain in every direction are two caves in particular: one is situated near the top, at about 100 metres above the valley; the other is near the base, at about 20 metres above the river. They both open towards the north. In the upper cave M. Fontan found a sandy loam with pebbles (the pebbles being of rocks different from that of the mountain), extending inwards for 100 metres, and containing a large quantity of bones of *Carnivora*, *Ruminantia*, and *Rodentia*,—those of the great Cave-bear, a large *Hyæna*, and a large *Felis* being the most numerous. On the surface some fragments of pottery, an iron poignard, and two Roman coins were found, with a quantity of cinders and charcoal; and at a depth of more than 3 feet in the ossiferous loam another bed of cinders and charcoal was met with, and in this M. Fontan found a bone arrow-head and two human teeth; the latter were at a distance of 5 or 6 metres one from the other.

In the lower cavern a blackish earth, with large granitic and other pebbles, was found to contain bones of the Red Deer, Antelope,

Aurochs, and Lynx; also worked flints and numerous utensils of bone (of deer chiefly), such as bodkins and arrows; the latter have grooves on their barbs, probably for poison. Some of the bones bear marks made of incisions by sharp instruments in flaying or cutting up the carcases. In each cavern a chasm crosses the gallery and terminates the deposits—in the upper cave at 100 metres, in the lower one at about 7 metres from the entrance.

The author argues that, from the facts which he has noticed, these caverns must have been subjected simultaneously to the effects of a great transient diluvial cataclysm coming from the N.N.W. or West, in the opposite direction to the present course of the waters of that region; that man and all the other animals the remains of which are buried in these caves existed in the valley before this inundation; and that the greater part of the animals inhabited the caves, but that man was not contemporary with all of them.

2. "Notes on some further Discoveries of Flint Implements in the Drift; with a few suggestions for search elsewhere." By J. Prestwich, Esq., F.R.S., Treas.G.S.

Since the author's communication to the Royal Society last year on the discovery of Flint Implements in Pleistocene beds at Abbeville, Amiens, and Hoxne, similar implements have been found in some new localities in this country.

In Suffolk, between Icklingham and Mildenhall, Mr. Warren has met with some specimens in the gravel of Rampart Hill in the valley of the Lark. This gravel is of later date than the Boulder-clay of the neighbourhood. In Kent, Mr. Leech, Mr. Evans, and the author found some specimens at the foot of the cliffs between Herne Bay and the Reculvers. The author believes them to have been derived from a freshwater deposit that caps the cliff, and which has been found by Mr. Evans and himself to yield similar specimens at Swale Cliff near Whitstable. In Bedfordshire, Mr. J. Wyatt, F.G.S., has found some specimens in the gravel at Biddenham, near Bedford; this gravel also is of freshwater origin, and is younger than the Boulder-clay. In Surrey, a specimen found in the gravel of Peasemarsh twenty-five years ago has been brought forward by its discoverer, Mr. Whitburn of Guildford. In Herts, Mr. Evans has found a specimen in the surface-drift on the Chalk Hills near Abbots Langley. Lastly, the author recommended that diligent search be made in the gravel and brick-earth at Copford and Lexden near Colchester, at Grays and Ilford in Essex, at Erith, Brentford, Taplow, Hurley, Reading, Oxford, Cambridge, Chippenham, Bath, Blandford, Salisbury, Chichester, Selsea, Peasemarsh, Godalming, Croydon, Hertford, Stamford, Orton near Peterborough, &c.

3. "On the *Corbicula* (or *Cyrena fluminalis*) geologically considered." By J. Gwyn Jeffreys, F.R.S., F.G.S.

Mr. Jeffreys has identified the species of *Corbicula*, found by Mr. Prestwich in a raised sea-beach at Kelsey Hill in Yorkshire, with that of the Grays deposit, as well as with the recent species from the

Euphrates and the Nile. He mentioned the great tendency to variation in freshwater shells, and the distribution of the same species throughout different and widely separated parts of the world; and he therefore considered that there was no difficulty in supposing that the *Corbicula* was contemporaneous in this country with Arctic shells found with it at Kelsey Hill. According to Mr. Jeffreys, specimens of Testacea from the north are larger than those of the same species from southern localities.

XXII. *Intelligence and Miscellaneous Articles.*

PHOTOGRAPHIC MICROMETER.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN, Parsonstown, July 1861.

I SAW last week in some newspaper a notice that in America micrometers have lately been made by means of photography. I do not know from what publication this notice has been taken; but as I have been for the last month or so engaged occasionally in making experiments with the same intention as an original idea, I send you a short account of what I have done, for publication if you think it sufficiently interesting.

My endeavour was to get a glass slide for a microscope marked so as to measure very minute objects; and as the micrometer I have (measuring $\frac{1}{1000}$th of an inch) was useless for the purpose I had in view, it occurred to me that by the diminishing power of the camera I might succeed in obtaining smaller divisions. I tried first for pictures of dark lines, $\frac{1}{10}$th of an inch in breadth, on a white ground, reduced to a small compass, but I did not succeed even with a very small aperture to the lens. I then substituted lines $\frac{1}{4}$th of an inch in breadth removed to a greater distance, and I got a pretty sharp picture; but I found that the sharpest and best-marked picture of distant lines I obtained was given by opake bars, placed so that the light from a clear sky came to the camera between them.

By nailing rods of blackened wood, $\frac{1}{4}$th of an inch broad and $\frac{1}{4}$th of an inch asunder, across a frame, and placing this at a suitable distance with a clear light behind, and using an aperture of about $\frac{1}{8}$th of an inch in diameter, I easily obtained well-marked and sharp lines the $\frac{1}{1000}$th of an inch apart and the $\frac{1}{10000}$th of an inch in breadth, sufficiently accurate for all the purposes of a micrometer. The picture of the lines requires to be covered with transparent varnish to prevent rubbing. I have taken the picture on very thin talc, and cemented it to glass with the collodion between the plates; and for object-glasses of small power I have found it answer; but the thickness of the talc is too much for the higher powers, as the object viewed and the lines do not sufficiently agree in focus.

I suppose the reason why lines with spaces between them give a better picture than black lines ruled on a white ground is because

there is no irradiation of light from behind, at least not nearly so much from the spaces as from the white ground. At all events, whatever the cause may be, I have found the lines with the spaces give a much better and sharper impression.

The picture of the lines should be a positive one, and very clear. I found the collodion prepared with the iodide of iron, according to the formula given in this Magazine, July 1854, to act admirably. It must be very sensitive, on account of the smallness of the aperture necessary for the required sharpness.

I have no doubt that much finer lines than these I have got might be obtained by the same process.

Your obedient Servant,

THOMAS WOODS, M.D.

ON THE BOILING OF LIQUIDS. BY M. L. DUFOUR.

The ebullition of liquids, instead of taking place under normal circumstances of temperature and pressure, varies, as is well known, with the vessel in which the liquid is contained. In an earthen vessel, for instance, the ebullition is at a higher point than in one of metal; and Marcet has shown that the treatment the glass has experienced, washing with sulphuric acid, &c., often modifies the boiling-point to the extent of several degrees. Water deprived of air and placed in the conditions of a *water-hammer*, may be heated several degrees above 100° C. without passing into the gaseous state, but it then boils violently. Donny has shown that water free from air and carefully heated, may be raised to 135° without assuming the gaseous state. This retardation of ebullition is further found in other liquids; and the violent production of vapour is a frequent indication of it in glass vessels.

The ebullition not being produced except at a temperature higher than that at which the elastic force of the vapour is equal to the external pressure, is due to two causes,—first, the adhesion of the liquid to the substance of the vessel; and secondly, the absence of air in solution.

There are nevertheless some curious cases in which the retardation of boiling cannot be explained by the adhesion to a solid, and the absence of air, yet where the contact of a solid produces a sudden formation of vapour. If linseed oil be heated in a dish to 105° or 110° and a few drops of water be allowed to fall, they will sink to the bottom of the vessel. The moment they touch there is a sudden formation of vapour; the globule of water, slightly diminished, is repelled a few millimetres from the bottom; it again sinks, giving rise to a fresh disengagement of vapour, which raises it again, and so on. There is no perceptible evaporation from the globules of water so long as they float on the oil, and are not in contact with the side of the vessel; and it is only on the sudden contact of the solid that a bubble of vapour is suddenly produced. It is natural to inquire what would take place if the water during its being heated

was kept from the side of the vessel, and floated in a medium of the same density as its own. The medium to be employed ought to exceed 100° without boiling, have the same density as water, and not form aqueous mixtures. Oils are unsuitable, but certain essences realize these conditions.

Essence of cloves, to which a small quantity of oil has been added, forms a liquid in which water remains in equilibrium in round spheres, and perfectly moveable in the interior. If heated carefully, a temperature far above 100° may frequently be attained before the ebullition of the water ensues. A temperature of 120° and 130° is frequently reached; and I have often had these aqueous spheres 10 millims. in diameter at 140° to 150°. Smaller spheres, 1 to 2 millims. in diameter, have often been raised to 170°, and even 175°; that is, to temperatures at which the tension of aqueous vapour is 8 atmospheres. The water had undergone no preparation; it was neither distilled nor free from air. At these high temperatures there is not, as might be thought, a slow and continuous ebullition. The spheres are as limpid and calm at 150° as at 10°.

Ebullition ensues when the globules come in contact with a solid. If, carried by the currents which are produced during the heating, they strike against the side of the vessel or the bulb of the thermometer, there is a sudden production of vapour. The globule, which has become somewhat smaller, is driven to some distance from the point at which the explosion is produced, but it continues to float. If, when the temperature exceeds 115° to 120°, the aqueous globule is touched with a glass or metal rod, a similar effect is produced; an explosion takes place at the point of contact, a bubble of vapour is disengaged which traverses the essence, and the globule is driven away as if the point had exerted on it a sudden repulsion. The solids best fitted for producing this effect are a pointed piece of wood or of charcoal. Glass or metal rods occasionally fail; the contact of saline crystals is generally successful.

The preceding phenomena may also be produced with other liquids when heated under suitable conditions. Chloroform, for example, heated in a solution of chloride of zinc, may be raised to 90° or 100°.

It is natural to connect these phenomena with those in which the contact of a solid induces the crystallization of supersaturated saline solutions, as well as with the sudden solidification of water, sulphur, &c., reduced below the ordinary temperature of solidification. They are also intimately connected with the phenomenon of liquids resisting solidification when they are immersed in a fluid medium. It appears as if the contact of solids were a determining cause for the change of condition in liquids; and it may be that the limits of temperature which we have assigned to the different conditions of bodies are less absolute than they appear.—*Comptes Rendus*, May 13, 1861.

THE

LONDON, EDINBURGH AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FOURTH SERIES.]

SEPTEMBER 1861.

XXIII. *On the Absorption and Radiation of Heat by Gases and Vapours, and on the Physical Connexion of Radiation, Absorption, and Conduction.—The Bakerian Lecture.* By JOHN TYNDALL *Esq., F.R.S. &c.**

[With a Plate.]

§ 1. THE researches on glaciers which I have had the honour of submitting from time to time to the notice of the Royal Society, directed my attention in a special manner to the observations and speculations of De Saussure, Fourier, M. Pouillet, and Mr. Hopkins, on the transmission of solar and terrestrial heat through the earth's atmosphere. This gave practical effect to a desire which I had previously entertained to make the mutual action of radiant heat and gases of all kinds the subject of an experimental inquiry.

Our acquaintance with this department of Physics is exceedingly limited. So far as my knowledge extends, the literature of the subject may be stated in a few words.

From experiments with his admirable thermo-electric apparatus, Melloni inferred that for a distance of 18 or 20 feet the absorption of radiant heat by atmospheric air is perfectly insensible†.

With a delicate apparatus of the same kind, Dr. Franz of Berlin found that the air contained in a tube 3 feet long absorbed 3·54 per cent. of the heat sent through it from an Argand lamp; that is to say, calling the number of rays which passed through the exhausted tube 100, the number which passed when the tube was filled with air was only 96·46‡.

* From the Philosophical Transactions, Part I. for 1861, having been read at the Royal Society February 7, 1861.

† *La Thermochrose*, p. 136. ‡ Pogg. *Ann.* vol. xciv. p. 342.

In the sequel I shall refer to circumstances which induce me to conclude that the result obtained by Dr. Franz is due to an inadvertence in his mode of observation. These are the only experiments of this nature with which I am acquainted, and they leave the field of inquiry now before us perfectly unbroken ground.

§ 2. At an early stage of the investigation, I experienced the need of a first-class galvanometer. My instrument was constructed by that excellent workman, Sauerwald of Berlin. The needles are suspended independently of the shade; the latter is constructed so as to enclose the smallest possible amount of air, the disturbance of aërial currents being thereby practically avoided. The plane glass plate, which forms the cover of the instrument, is close to the needle; so that the position of the latter can be read off with ease and accuracy either by the naked eye or by a magnifying lens.

The wire of the coil belonging to this instrument was drawn from copper obtained from a galvano-plastic manufactory in the Prussian Capital; but it was not free from the magnetic metals.

In consequence of its impurity in this respect, when the needles were perfectly astatic they deviated as much as 30° right and left of the neutral line. To neutralize this, a "compensator" was made use of, by which the needle was gently drawn to zero in opposition to the magnetism of the coil.

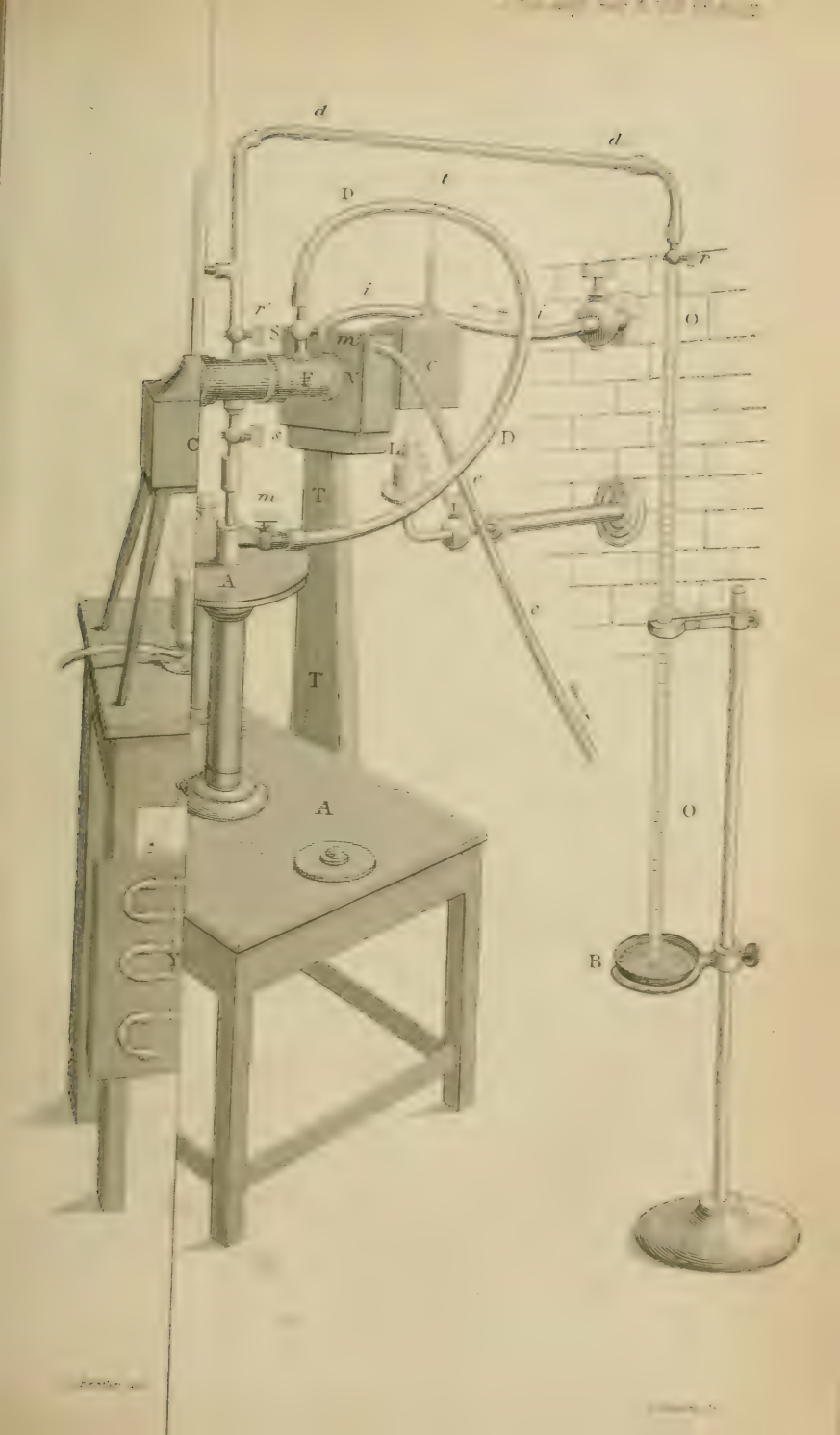
But the instrument suffered much in point of delicacy from this arrangement, and accurate quantitative determinations with it were unattainable. I therefore sought to replace the Berlin coil by a less magnetic one. Mr. Becker first supplied me with a coil which reduced the lateral deflection from 30° to 3°.

But even this small residue was a source of great annoyance to me; and for a time I almost despaired of obtaining pure copper wire. I knew that Professor Magnus had succeeded in obtaining it for his galvanometer, but the labour of doing so was immense*. Previous to undertaking a similar task, the thought occurred to me, that for my purpose a magnet furnished an immediate and perfect test as to the quality of the wire. Pure copper is *diamagnetic*; hence its repulsion or attraction by the magnet would at once declare its fitness or unfitness for the purpose which I had in view.

Fragments of the wire first furnished to me by M. Sauerwald were strongly attracted by the magnet. The wire furnished by Mr. Becker, when covered with its green silk, was also attracted, though in a much feebler degree.

I then removed the green silk covering from the latter and tested the naked wire. *It was repelled.* The whole annoyance

* Pogg. *Ann.* vol. lxxxiii. p. 489; and Phil. Mag. 1852, vol. iii. p. 82.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was thus fastened on the green silk; some iron compound had been used in the dyeing of it; and to this the deviation of my needle from zero was manifestly due.

I had the green coating removed and the wire overspun with white silk, clean hands being used in the process. A perfect galvanometer is the result. The needle, when released from the action of a current, returns accurately to zero, and is perfectly free from all magnetic action on the part of the coil. In fact while we have been devising agate plates and other elaborate methods to get rid of the great nuisance of a magnetic coil*, the means of doing so are at hand. Nothing is more easy to be found than diamagnetic copper wire. Out of eleven specimens, four of which were furnished by Mr. Becker, and seven taken at random from our laboratory, nine were found diamagnetic and only two magnetic.

Perhaps the only defect of those fine instruments with which Du Bois Raymond conducts his admirable researches in animal electricity is that above alluded to. The needle never comes to zero, but is drawn to it by a minute magnet. This defect may be completely removed. By the substitution of clean white silk for green, however large the coil may be, the compensator may be dispensed with, and a great augmentation of delicacy secured. The instrument will be rendered suitable for quantitative measurements; effects which are now beyond the reach of experiment will be rendered manifest; while the important results hitherto established will be obtained with a fraction of the length of wire now in use†.

§ 3. Our present knowledge of the deportment of liquids and solids, would lead to the inference that, if gases and vapours exercised any appreciable absorptive power on radiant heat, the absorption would make itself most manifest on heat emanating from an obscure source. But an experimental difficulty occurs at the outset in dealing with such heat. How must we close the receiver containing the gases through which the calorific rays are to be sent? Melloni found that a glass plate one-tenth of an inch in thickness intercepted all the rays emanating from a source of the temperature of boiling water, and fully 94 per cent. of the rays from a source of 400° Centigrade. Hence a tube closed with glass plates would be scarcely more suitable for the purpose now under consideration, than if its ends were stopped by plates of metal.

* See Melloni upon this subject, *Thermochrose*, pp. 31–33.

† Mr. Becker, to whose skill and intelligence I have been greatly indebted, furnished me with several specimens of wire of the same fineness as that used by Du Bois Raymond, some covered with green silk and others with white. The former were invariably attracted, the latter invariably repelled. In all cases the *naked* wire was repelled.

Rock-salt immediately suggests itself as the proper substance; but to obtain plates of suitable size and transparency was exceedingly difficult. Indeed, had I been less efficiently seconded, the obstacles thus arising might have been insuperable. To the Trustees of the British Museum I am indebted for the material of one good plate of salt; to Mr. Harlin for another; while Mr. Lettsom, at the instance of Mr. Darker*, brought me a piece of salt from Germany from which two fair plates were taken. To Lady Murchison, Sir Emerson Tennant, Sir Philip Egerton, and Mr. Pattison my best thanks are also due for their friendly assistance.

The first experiments were made with a tube of tin polished inside, 4 feet long and 2·4 inches in diameter, the ends of which were furnished with brass appendages to receive the plates of rock-salt. Each plate was pressed firmly against a flange by means of a bayonet joint, being separated from the flange by a suitable washer. Various descriptions of leather washers were tried for this purpose and rejected. The substance finally chosen was vulcanized india-rubber very lightly smeared with a mixture of bees-wax and spermaceti. A T-piece was attached to the tube, communicating on one side with a good air-pump, and on the other with the external air, or with a vessel containing the proper gas.

The tube being mounted horizontally, a Leslie's cube containing hot water was placed close to one of its ends, while an excellent thermo-electric pile, connected with its galvanometer, was presented to the other. The tube being exhausted, the calorific rays sent through it fell upon the pile, a permanent deflection of 30° being the consequence. The temperature of the water was in the first instance purposely so arranged as to produce this deflection.

Dry air was now admitted into the tube, while the needle of the galvanometer was observed with all possible care. Even by the aid of a magnifying lens I could not detect the slightest change of position. Oxygen, hydrogen, and nitrogen, subjected to the same test, gave the same negative result. The temperature of the water was subsequently lowered so as to produce a deflection of 20° and 10° in succession, and then heightened till the deflection amounted to 40°, 50°, 60° and 70°; but in no case did the admission of air, or any of the above gases into the exhausted tube, produce any sensible change in the position of the needle.

It is a well-known peculiarity of the galvanometer, that its higher and lower degrees represent different amounts of calorific

* During the course of the inquiry, I have often had occasion to avail myself of the assistance of this excellent mechanician.

action. In my instrument, for example, the quantity of heat necessary to move the needle from 60° to 61° is about twenty times that required to move it from 11° to 12°. Now in the case of the small deflections above referred to, the needle was, it is true, in a sensitive position; but then the total amount of heat passing through the tube was so inconsiderable that a small per-centage of it, even if absorbed, might well escape detection. In the case of the large deflections, on the other hand, though the total amount of heat was large, and though the quantity absorbed might be proportionate, the needle was in such a position as to require a very considerable abstraction of heat to produce any sensible change in its position. Hence arose the thought of operating, if possible, with large quantities of heat, while the needle intended to reveal its absorption should continue to occupy its position of maximum delicacy.

The first attempt at solving this problem was as follows:—My galvanometer is a differential one—the coil being composed of two wires wound side by side, so that a current could be sent through either of them independent of the other. The thermo-electric pile was placed at one end of the tin tube, and the ends of one of the galvanometer wires connected with it. A copper ball heated to low redness being placed at the other end of the tube, the needle of the galvanometer was propelled to its stops near 90°. The ends of the second wire were now so attached to a second pile that when the latter was caused to approach the copper ball, the current thus excited passed through the coil in a direction opposed to the first one. Gradually, as the second pile was brought nearer to the source of heat, the needle descended from the stops, and when the two currents were nearly equal the position of the needle was close to zero.

Here then we had a powerful flux of heat through the tube; and if a column of gas four feet long exercised any sensible absorption, the needle was in the position best calculated to reveal it. In the first experiment made in this way, the neutralization of one current by the other occurred when the tube was filled with air; and after the exhaustion of the tube had commenced, the needle started suddenly off in a direction which indicated that a *less* amount of heat passed through the partially exhausted tube, than through the tube filled with air. The needle, however, soon stopped, turned, descended quickly to zero, and passed on to the other side, where its deflection became permanent. The air made use of in this experiment came direct from the laboratory, and the first impulsion of the needle was probably due to the aqueous vapour precipitated as a cloud by the sudden exhaustion of the tube. When, previous to its admission, the air was passed over chloride of calcium, or

pumice-stone moistened with sulphuric acid, no such effect was observed. The needle moved steadily in one direction until its maximum deflection was attained, and this deflection showed that in all cases radiant heat was absorbed by the air within the tube.

These experiments were commenced in the spring of 1859, and continued without intermission for seven weeks. The course of the inquiry during this whole period was an incessant struggle with experimental difficulties. Approximate results were easily obtainable; but I aimed at exact measurements, which could not be made with a varying source of heat like the copper ball. I resorted to copper cubes containing fusible metal, or oil, raised to a high temperature; but was not satisfied with their action. I finally had a lamp constructed which poured a sheet of gas-flame along a plate of copper; and to keep the flame constant, a gas regulator specially constructed for me by Mr. Hulet was made use of. It was also arranged that the radiating plate should form one of the walls of a chamber which could be connected with the air-pump and exhausted, so that the heat emitted by the copper plate might cross a vacuum before entering the experimental tube. With this apparatus I determined approximately the absorption of nine gases and twenty vapours during the summer of 1859. The results would furnish materials for a long memoir; but increased experience and improved methods have enabled me to substitute for them others of greater value; I shall therefore pass over the work of these seven weeks without further allusion to it.

On the 9th of September of the present year (1860) I resumed the inquiry. For three weeks I worked with the plate of copper as my source of heat, but finally rejected it on the score of insufficient constancy. I again resorted to the cube of hot oil, and continued to work with it up to Monday the 29th of October. During the seven weeks just referred to, I experimented from eight to ten hours daily; but these experiments, though more accurate, must unhappily share the fate of the former ones. In fact the period was one of discipline—a continued struggle against the difficulties of the subject and the defects of the locality in which the inquiry was conducted.

My reason for making use of the high sources of heat above referred to was, that the absorptive power of some of the gases which I had examined was so small that, to make it clearly evident, a high temperature was essential. For other gases, and for *all* the vapours that had come under my notice, a source of lower temperature would have been not only sufficient, but far preferable. I was finally induced to resort to boiling water, which, though it gave greatly diminished effects, was capable of being preserved at so constant a temperature that deflections

which, with the other sources, would be masked by the errors of observation, became with it true quantitative measures of absorption.

§ 4. The entire apparatus made use of in the experiments on absorption is figured on Plate III. S S′ is the *experimental tube*, composed of brass, polished within, and connected, as shown in the figure, with the air-pump, A A. At S and S′ are the plates of rock-salt which close the tube air-tight. The length from S to S′ is 4 feet. C is a cube containing boiling water, in which is immersed the thermometer *t*. The cube is of cast copper, and on one of its faces a projecting ring was cast to which a brass tube of the same diameter as S S′, and capable of being connected air-tight with the latter, was carefully soldered. The face of the cube within the ring is the radiating plate, which is coated with lampblack. Thus between the cube C and the first plate of rock-salt there is *a front chamber* F, connected with the air-pump by the flexible tube D D, and capable of being exhausted independently of S S′. To prevent the heat of conduction from reaching the plate of rock-salt S, the tube F is caused to pass through a vessel V, being soldered to the latter where it enters it and issues from it. This vessel is supplied with a continuous flow of cold water through the influx tube *i i*, which dips to the bottom of the vessel; the water escapes through the efflux tube *e e*, and the continued circulation of the cold liquid completely intercepts the heat that would otherwise reach the plate S.

The cube C is heated by the gas-lamp L. P is the thermo-electric pile placed on its stand at the end of the experimental tube, and furnished with two conical reflectors, as shown in the figure. C′ is the *compensating cube*, used to neutralize by its radiation* the effect of the rays passing through S S′. The regulation of this neutralization was an operation of some delicacy; to effect it the double screen H was connected with a winch and screw arrangement, by which it could be advanced or withdrawn through extremely minute spaces. For this most useful adjunct I am indebted to the kindness of my friend Mr. Gassiot. N N is the galvanometer, with perfectly astatic needles and perfectly non-magnetic coil; it is connected with the pile P by the wires *w w*; Y Y is a system of six chloride-of-calcium tubes, each 32 inches long; R is a U-tube containing fragments of pumice-stone, moistened with strong caustic potash; and Z is a second similar tube, containing fragments of pumice-stone wetted with strong sulphuric acid. When *drying* only was aimed at, the potash tube was suppressed. When, on

* It will be seen that in this arrangement I have abandoned the use of the differential galvanometer, and made the thermo-electric pile the differential instrument.

the contrary, as in the case of atmospheric air, both moisture and carbonic acid were to be removed, the potash tube was included. G G is a holder from which the gas to be experimented with was sent through the drying-tubes, and thence through the pipe *p p* into the experimental tube S S′. The appendage at M and the arrangement at O O may for the present be disregarded; I shall refer to them particularly by and by.

The mode of proceeding was as follows:—The tube S S′ and the chamber F being exhausted as perfectly as possible, the connexion between them was intercepted by shutting off the cocks *m*, *m′*. The rays from the interior blackened surface of the cube C passed first across the vacuum F, then through the plate of rock-salt S, traversed the experimental tube, crossed the second plate S′, and being concentrated by the anterior conical reflector, impinged upon the adjacent face of the pile P. Meanwhile the rays from the hot cube C′ fell upon the opposite face of the pile, and the position of the galvanometer needle declared at once which source was predominant. A movement of the screen H back or forward with the hand sufficed to establish an approximate equality; but to make the radiations perfectly equal, and thus bring the needle exactly to 0°, the fine motion of the screw above referred to was necessary. The needle being at 0°, the gas to be examined was admitted into the tube; passing, in the first place, through the drying apparatus. Any required quantity of the gas may be admitted; and here experiments on gases and vapours enjoy an advantage over those with liquids and solids, namely, the capability of changing the density at pleasure. When the required quantity of gas had been admitted, the galvanometer was observed, and from the deflection of its needle the absorption was accurately determined.

Up to about its 36th degree, the degrees of my galvanometer are all equal in value; that is to say, it requires the same amount of heat to move the needle from 1° to 2° as to move it from 35° to 36°. Beyond this limit the degrees are equivalent to larger amounts of heat. The instrument was accurately calibrated by the method recommended by Melloni (*Thermochrose*, p. 59); so that the precise value of its larger deflections are at once obtained by reference to a table. Up to the 36th degree, therefore, the simple deflections may be regarded as the expression of the absorption; but beyond this the absorption equivalent to any deflection is obtained from the table of calibration.

§ 5. The air of the laboratory, freed from its moisture and carbonic acid, and permitted to enter until the tube was filled, produced a deflection of about . . . . . 1°.

Oxygen obtained from chlorate of potash and peroxide of manganese produced a deflection of about . . . . . 1°.

One specimen of nitrogen, obtained from the decomposition of nitrate of potash, produced a deflection of about . . . 1°.

Hydrogen from zinc and sulphuric acid produced a deflection of about . . . . . 1°.

Hydrogen obtained from the electrolysis of water produced a deflection of about . . . . . 1°.

Oxygen obtained from the electrolysis of water, and sent through a series of eight bulbs containing a strong solution of iodide of potassium, produced a deflection of about . . 1°.

In the last experiment the electrolytic oxygen was freed from its ozone. The iodide of potassium was afterwards suppressed, and the oxygen, plus its ozone, admitted into the tube; the deflection produced was . . . . . 4°.

Hence the small quantity of ozone which accompanied the oxygen in this case trebled the absorption of the oxygen itself*.

I have repeated this experiment many times, employing different sources of heat. With sources of high temperature the difference between the ozone and the ordinary oxygen comes out very strikingly. By careful decomposition a much larger amount of ozone might be obtained, and a corresponding large effect on radiant heat produced.

In obtaining the electrolytic oxygen, I made use of two different vessels. To diminish the resistance of the acidulated water to the passage of the current, I placed in one vessel a pair of very large platinum plates, between which the current from a battery of ten of Grove's cells was transmitted. The oxygen bubbles liberated on so large a surface were extremely minute, and the gas thus generated, on being sent through iodide of potassium, scarcely coloured the liquid; the characteristic odour of ozone was also almost entirely absent. In the second vessel smaller plates were used. The bubbles of oxygen were much larger, and did not come into such intimate contact with either the platinum or the water. The oxygen thus obtained showed the characteristic reactions of ozone; and with it the above result was obtained.

The total amount of heat transmitted through the tube in these experiments produced a deflection of . . . . 71°·5.

Taking as unit of heat the quantity necessary to cause the needle to move from 0° to 1°, the number of units expressed by the above deflection is . . . . . 308.

Hence the absorption by the above gases amounted to about 0·33 per cent.

I am unable at the present moment to range with certainty oxygen, hydrogen, nitrogen, and atmospheric air in the order of

* It will be seen further on that this result is in harmony with the supposition that ozone, obtained in the manner described, is a *compound* body.

their absorptive powers, though I have made several hundred experiments with the view of doing so. Their proper action is so small that the slightest foreign impurity gives one a predominance over the other. In preparing the gases, I have resorted to the methods which I found recommended in chemical treatises, but as yet only to discover the defects incidental to these methods. Augmented experience and the assistance of my friends will, I trust, enable me to solve this point by and by. An examination of the whole of the experiments induces me to regard hydrogen as the gas which exercises the lowest absorptive power.

We have here the cases of minimum gaseous absorption. It will be interesting to place in juxtaposition with the above results some of those obtained with olefiant gas—the most highly absorbent permanent gas that I have hitherto examined. I select for this purpose an experiment made on the 21st of November.

The needle being steady at zero in consequence of the equality of the actions on the opposite faces of the pile, the admission of olefiant gas gave a permanent deflection of . . 70°·3.

The gas being completely removed, and the equilibrium reestablished, a plate of polished metal was interposed between one of the faces of the pile and the source of heat adjacent. The total amount of heat passing through the exhausted tube was thus found to produce a deflection of . . . . . 75°.

Now a deflection of 70°·3 is equivalent to 290 units, and a deflection of 75° is equivalent to 360 units; hence more than seven-ninths of the total heat was cut off by the olefiant gas, or about 81 per cent.

The extraordinary energy with which the needle was deflected when the olefiant gas was admitted into the tube, was such as might occur had the plates of rock-salt become suddenly covered with an opake layer. To test whether any such action occurred, I polished a plate carefully, and projected against it for a considerable time a stream of the gas; there was no dimness produced. The plates of rock-salt, moreover, which were removed daily from the tube, usually appeared as bright when taken out as when they were put in.

The gas in these experiments issued from its holder, and had there been in contact with cold water. To test whether it had chilled the plates of rock-salt, and thus produced the effect, I filled a similar holder with atmospheric air and allowed it to attain the temperature of the water; but its action was not thereby sensibly augmented.

In order to subject the gas to ocular examination, I had a glass tube constructed and connected with the air-pump. On permitting olefiant gas to enter it, not the slightest dimness or

opacity was observed. To remove the last trace of doubt as to the possible action of the gas on the plates of rock-salt, the tin tube referred to at the commencement was perforated at its centre and a cock inserted into it; the source of heat was at one end of the tube, and the thermo-electric pile at some distance from the other. The plates of salt were entirely abandoned, the tube being open at its ends and consequently full of air. On allowing the olefiant gas to stream for a second or two into the tube through the central cock, the needle flew off and struck against its stops. It was held steadily for a considerable time between 80° and 90°.

A slow current of air sent through the tube gradually removed the gas, and the needle returned accurately to zero.

The gas within the holder being under a pressure of about 12 inches of water, the cock attached to the cube was turned quickly on and off; the quantity of gas which entered the tube in this brief interval was sufficient to cause the needle to be driven to the stops, and steadily held between 60° and 70°.

The gas being again removed, the cock was turned once half round as quickly as possible. The needle was driven in the first instance through an arc of 60°, and was held permanently at 50°.

The quantity of gas which produced this last effect, on being admitted into a graduated tube, was found not to exceed one-sixth of a cubic inch in volume.

The tube was now taken away, and both sources of heat allowed to act from some distance on the thermo-electric pile. When the needle was at zero, olefiant gas was allowed to issue from a common argand burner into the air between one of the sources of heat and the pile. The gas was invisible, nothing was seen in the air, but the needle immediately declared its presence, being driven through an arc of 41°. In the four experiments last described, the source of heat was a cube of oil heated to 250° Centigrade, the compensation cube being filled with boiling water*.

Those who like myself have been taught to regard transparent gases as almost perfectly diathermanous, will probably share the astonishment with which I witnessed the foregoing effects. I was indeed slow to believe it possible that a body so constituted, and so transparent to light as olefiant gas, could be so densely opake to any kind of calorific rays; and to secure myself against error, I made several hundred experiments with this single substance. By citing them at greater length, however, I do not think I

*. With a cube containing boiling water I have since made this experiment visible to a large audience.

could add to the conclusiveness of the proofs just furnished, that the case is one of true calorific absorption*.

§ 6. Having thus established in a general way the absorptive power of olefiant gas, the question arises, "What is the relation which subsists between the density of the gas and the quantity of heat extinguished?"

I sought at first to answer this question in the following way:—An ordinary mercurial gauge was attached to the air-pump; the experimental tube being exhausted, and the needle of the galvanometer at zero, olefiant gas was admitted until it depressed the mercurial column 1 inch, the consequent deflection being noted; the gas was then admitted until a depression of 2 inches was observed, and thus the absorption effected by gas of 1, 2, 3, and more inches tension was determined. In the following Table the first column contains the tensions in inches, the second the deflections, and the third the absorption equivalent to each deflection.

Table I.—Olefiant Gas.

Tensions in inches.	Deflections.	Absorption.
1	56°	90
2	58·2	123
3	59·3	142
4	60·0	157
5	60·5	168
6	61·0	177
7	61·4	182
8	61·7	186
9	62·0	190
10	62·2	192
20	66·0	227

No definite relation between the density of the gas and its absorption is here exhibited. We see that an augmentation of the density *seven times* about *doubles* the amount of the absorption; while gas of 20 inches tension effects only $2\frac{1}{2}$ times the absorption of gas possessing 1 inch of tension.

But here the following reflections suggest themselves:—It is evident that olefiant gas of 1 inch tension, producing so large a deflection as 56°, must extinguish a large proportion of the rays which are capable of being absorbed by the gas, and hence the succeeding measures having a less and less amount of heat to act upon must produce a continually smaller effect. But sup-

* It is evident that the old mode of experiment might be applied to this gas. Indeed, several of the solids examined by Melloni are inferior to it in absorptive power. Had time permitted, I should have checked my results by experiments made in the usual way; this I intend to do on a future occasion.

posing the quantity of gas first introduced to be so inconsiderable that the number of rays extinguished by it is a vanishing quantity compared with the total number capable of absorption, we might reasonably expect that in this case a double quantity of gas would produce a double effect, a treble quantity a treble effect, or in general terms, that the absorption would, for a time, be proportional to the density

To test this idea, a portion of the apparatus, which was purposely omitted in the description already given, was made use of. O O, Plate III., is a graduated glass tube, the end of which dips into the basin of water B. The tube can be stopped above by means of the stopcock r; $d\,d$ is a tube containing fragments of chloride of calcium. The tube O O being first filled with water to the cock r, had this water displaced by olefiant gas; and afterwards the tube S S′, and the entire space between the cock r and the experimental tube, was exhausted. The cock n being now closed and r' left open, the cock r at the top of the tube O O was carefully turned on and the gas permitted to enter the tube S S′ with extreme slowness. The water rose in O O, each of whose smallest divisions represents a volume of $\frac{1}{50}$th of a cubic inch. Successive measures of this capacity were admitted into the tube and the absorption in each case determined.

In the following Table the first column contains the quantity of gas admitted into the tube; the second contains the corresponding deflection, which, within the limits of the Table, expresses the absorption; the third column contains the absorption, calculated on the supposition that it is proportional to the density.

Table II.—Olefiant Gas.

Unit-measure $\frac{1}{50}$th of a cubic inch.

Measures of gas.	Absorption. Observed.	Absorption. Calculated.
1	2·2	2·2
2	4·5	4·4
3	6·6	6·6
4	8·8	8·8
5	11·0	11·0
6	12·0	13·2
7	14·8	15·4
8	16·8	17·6
9	19·8	19·8
10	22·0	22·0
11	24·0	24·2
12	25·4	26·4
13	29·0	28·6
14	30·2	29·8
15	33·5	33·0

This Table shows the correctness of the foregoing surmise, and proves that for small quantities of gas the absorption is exactly proportional to the density.

Let us now estimate the tensions of the quantities of gas with which we have here operated. The length of the experimental tube is 48 inches, and its diameter 2·4 inches; its volume is therefore 218 cubic inches. Adding to this the contents of the cocks and other conduits which led to the tube, we may assume that each fiftieth of a cubic inch of the gas had to diffuse itself through a space of 220 cubic inches. The tension, therefore, of a single measure of the gas thus diffused would be $\frac{1}{11,000}$th of an atmosphere,—a tension capable of depressing the mercurial column connected with the pump $\frac{1}{367}$th of an inch, or about $\frac{1}{15}$th of a millimetre!

But the absorptive energy of olefiant gas, extraordinary as it is shown to be by the above experiments, is far exceeded by that of some of the vapours of volatile liquids. A glass flask was provided with a brass cap furnished with an interior thread, by means of which a stopcock could be screwed air-tight on to the flask. Sulphuric ether being placed in the latter, the space above the liquid was completely freed of air by means of a second air-pump. The flask, with its closed stopcock, was now attached to the experimental tube; the latter was exhausted and the needle brought to zero. The cock was then turned on so that the ether-vapour slowly entered the experimental tube. An assistant observed the gauge of the air-pump, and when it had sunk an inch, the stopcock was promptly closed. The galvanometric deflection consequent on the partial cutting off of the calorific rays was then noted; a second quantity of the vapour, sufficient to depress the gauge another inch, was then admitted, and in this way the absorptions of five successive measures, each possessing within the tube 1 inch of tension, were determined.

In the following Table the first column contains the tensions in inches, the second the deflection due to each, and the third the amount of heat absorbed, expressed in the units already referred to. For the purpose of comparison I have placed the corresponding absorption of olefiant gas in the fourth column.

Table III.—Sulphuric Ether.

Tensions in inches.	Deflections.	Absorption.	Corresponding absorption by olefiant gas.
1	64·8	214	90
2	70·0	282	123
3	72·0	315	142
4	73·0	330	154
5	73·0	330	163

For these tensions the absorption of radiant heat by the vapour of sulphuric ether is more than twice the absorption of olefiant gas. We also observe that in the case of the former the successive absorptions approximate more quickly to a ratio of equality. In fact the absorption produced by 4 inches of the vapour was sensibly the same as that produced by 5.

But reflections similar to those which we have already applied to olefiant gas are also applicable to ether. Supposing we make our unit-measure small enough, the number of rays first destroyed will vanish in comparison with the total number, and for a time the fact will probably manifest itself that the absorption is directly proportional to the density. To examine whether this is the case, the other portion of the apparatus, omitted in the general description, was made use of. K is a small flask with a brass cap, which is closely screwed on to the stopcock c'. Between the cocks c' and c, which latter is connected with the experimental tube, is the chamber M, the capacity of which was accurately determined. The flask k was partially filled with ether, and the air above the liquid removed. The stopcock c' being shut off and c turned on, the tube S S′ and the chamber M are exhausted. The cock c is now shut off, and c' being turned on, the chamber M becomes filled with pure ether vapour. By turning c' off and c on, this quantity of vapour is allowed to diffuse itself through the experimental tube, and its absorption determined; successive measures are thus sent into the tube, and the effect produced by each is noted. Measures of various capacities were made use of, according to the requirements of the vapours examined.

In the first series of experiments made with this apparatus, I omitted to remove the air from the space above the liquid; each measure therefore sent in to the tube was a mixture of vapour and air. This diminished the effect of the former; but the proportionality, for small quantities, of density to absorption exhibits itself so decidedly as to induce me to give the observations. The first column, as usual, contains the measures of vapour, the second the observed absorption, and the third the calculated absorption. The galvanometric deflections are omitted, their equivalents being contained in the second column. In fact as far as the eighth observation, the absorptions are merely the record of the deflections.

Table IV.—Mixture of Ether Vapour and Air.

Unit-measure $\frac{1}{30}$th of a cubic inch.

Measures.	Absorption.	
	Observed.	Calculated.
1	4·5	4·5
2	9·2	9·0
3	13·5	13·5
4	18·0	18·0
5	22·8	23·5
6	27·0	27·0
7	31·8	31·5
8	36·0	36·0
9	39·7	40·0
10	45·0	45·0
20	81 0	90·0
21	82·8	95·0
22	84·0	99·0
23	87·0	104·0
24	88·0	108·0
25	90·0	113·0
26	93·0	117·0
27	94·0	122·0
28	95·0	126·0
29	98·0	131·0
30	100·0	135·0

Up to the 10th measure we find that density and absorption augment in precisely the same ratio. While the former varies from 1 to 10, the latter varies from 4·5 to 45. At the 20th measure, however, a deviation from proportionality is apparent, and the divergence gradually augments from 20 to 30. In fact 20 measures tell upon the rays capable of being absorbed,—the quantity destroyed becoming so considerable, that every additional measure encounters a smaller number of such rays, and hence produces a diminished effect.

With ether vapour alone, the results recorded in the following Table were obtained. Wishing to determine the absorption exercised by vapour of very low tension, the capacity of the unit-measure was reduced to $\frac{1}{100}$th of a cubic inch.

TABLE V.—Sulphuric Ether.

Unit-measure $\frac{1}{100}$th of a cubic inch.

Measures.	Absorption. Observed.	Calculated.	Measures.	Absorption. Observed.	Calculated.
1	5·0	4·6	17	65·5	77·2
2	10·3	9·2	18	68·0	83·0
4	19·2	18·4	19	70·0	87·4
5	24·5	23·0	20	72·0	92·0
6	29·5	27·0	21	73·0	96·7
7	34·5	32·2	22	73·0	101·2
8	38·0	36·8	23	73·0	105·8
9	44·0	41·4	24	77·0	110·4
10	46·2	46·2	25	78·0	115·0
11	50·0	50·6	26	78·0	119·6
12	52·8	55·2	27	80·0	124·2
13	55·0	59·8	28	80·5	128·8
14	57·2	64·4	29	81·0	133·4
15	59·4	69·0	30	81·0	138·0
16	62·5	73·6			

We here find that the proportion between density and absorption holds sensibly good for the first eleven measures, after which the deviation gradually augments.

I have examined some specimens of ether which acted still more energetically on the thermal rays than those above recorded. No doubt for smaller measures than $\frac{1}{100}$th of a cubic inch the above law holds still more rigidly true; and in a suitable locality it would be easy to determine with perfect accuracy $\frac{1}{10}$th of the absorption produced by the first measure; this would correspond to $\frac{1}{1000}$th of a cubic inch of vapour. But on entering the tube the vapour had only the tension due to the temperature of the laboratory, namely 12 inches. This would require to be multiplied by 2·5 to bring it up to that of the atmosphere. Hence the $\frac{1}{1000}$th of a cubic inch, the absorption of which I have affirmed to be capable of measurement, would, on being diffused through a tube possessing a capacity of 220 cubic inches, have a tension of $\frac{1}{220} \times \frac{1}{2 \cdot 5} \times \frac{1}{1000} = \frac{1}{500,000}$th part of an atmosphere!

I have now to record the results obtained with thirteen other vapours. The method of experiment was in all cases the same as that just employed in the case of ether, the only variable element being the size of the unit-measure; for with many substances no sensible effect could be obtained with a unit volume so small as that used in the experiments last recorded. With bisulphide of carbon, for example, it was necessary to augment the unit-measure 50 times to render the measurements satisfactory.

Table VI.—Bisulphide of Carbon.

Unit-measure $\frac{1}{2}$ a cubic inch.

Measures.	Absorption. Observed.	Calculated.
1	2·2	2·2
2	4·9	4·4
3	6·5	6·6
4	8·8	8·8
5	10·7	11·0
6	12·5	13·0
7	13·8	15·4
8	14·5	17·6
9	15·0	19·0
10	15·6	22·0
11	16·2	24·2
12	16·8	26·4
13	17·5	28·6
14	18·2	30·8
15	19·0	33·0
16	20·0	35·2
17	20·0	37·4
18	20·2	39·6
19	21·0	41·8
20	21·0	44·0

As far as the sixth measure the absorption is proportional to the density; after which the effect of each successive measure diminishes. Comparing the absorption effected by a quantity of vapour which depressed the mercury column half an inch, with that effected by vapour possessing one inch of tension, the same deviation from proportionality is observed.

By mercurial gauge.

Tension.	Absorption.
$\frac{1}{2}$ inch	14·8
1 inch	18·8

These numbers simply express the galvanometric deflections, which, as already stated, are strictly proportional to the absorption as far as 36° or 37°. Did the law of proportion hold good, the absorption due to 1 inch of tension ought of course to be 29·6 instead of 18·8.

Whether for equal volumes of the vapours at their maximum density, or for equal tensions as measured by the depression of the mercurial column, bisulphide of carbon exercises the lowest absorptive power of all the vapours which I have hitherto examined. For very small quantities, a volume of sulphuric ether vapour, at its maximum density in the measure, and expanded thence into the tube, absorbs 100 times the quantity of radiant heat intercepted by an equal volume of bisulphide of carbon vapour at its maximum density. These are the extreme limits

of the scale, as far as my inquiries have hitherto proceeded. The action of every other vapour is less than that of sulphuric ether, and greater than that of bisulphide of carbon.

A very singular phenomenon was repeatedly observed during the experiments with bisulphide of carbon. After determining the absorption of the vapour, the tube was exhausted as perfectly as possible, the trace of vapour left behind being exceedingly minute. Dry air was then admitted to cleanse the tube. On again exhausting, after the first few strokes of the pump a jar was felt and a kind of explosion heard, while dense volumes of blue smoke immediately issued from the cylinders. The action was confined to the latter, and never propagated backwards into the experimental tube.

It is only with bisulphide of carbon that this effect has been observed. It may, I think, be explained in the following manner:—To open the valve of the piston, the gas beneath it must have a certain tension, and the compression necessary to produce this appears sufficient to cause the combination of the constituents of the bisulphide of carbon with the oxygen of the air. Such a combination certainly takes place, for the odour of sulphurous acid is unmistakeable amid the fumes.

To test this idea I tried the effect of compression in the air-syringe. A bit of tow or cotton wool moistened with bisulphide of carbon, and placed in the syringe, emitted a bright flash when the air was compressed. By blowing out the fumes with a glass tube, this experiment may be repeated twenty times with the same bit of cotton.

It is not necessary even to let the moistened cotton remain in the syringe. If the bit of tow or cotton be thrown into it, and out again as quickly as it can be ejected, on compressing the air the luminous flash is seen. Pure oxygen produces a brighter flash than atmospheric air. These facts are in harmony with the above explanation.

Table VII.—Amylene.

Unit-measure $\frac{1}{10}$th of a cubic inch.

	Absorption.	
Measures.	Observed.	Calculated.
1	3·4	4·3
2	8·4	8·6
3	12·0	12·9
4	16·5	17·2
5	21·6	21·5
6	26·5	25·8
7	30·6	30·1
8	35·3	34·4
9	39·0	38·7
10	44·0	43·0

For these quantities the absorption is proportional to the density, but for large quantities the usual deviation is observed, as shown by the following observations:—

By mercurial gauge.

Tension.	Deflection.	Absorption.
$\frac{1}{2}$ inch	60°	157
1 inch	65	216

Did the proportion hold good, the absorption for an inch of tension ought of course to be 314 instead of 216.

TABLE VIII.—Iodide of Ethyle.

Unit-measure $\frac{1}{10}$th of a cubic inch.

	Absorption.	
Measures.	Observed.	Calculated.
1	5·4	5·1
2	10·3	10·2
3	16·8	15·3
4	22·2	20·4
5	26·6	25·5
6	31·8	30·6
7	35·6	35·9
8	40·0	40·8
9	44·0	45·9
10	47·5	51·0

By mercurial gauge.

Tension.	Deflection.	Absorption.
$\frac{1}{2}$ inch	56°·3	94
1 inch	58·2	120

TABLE IX.—Iodide of Methyle.

Unit-measure $\frac{1}{10}$th of a cubic inch.

	Absorption.	
Measures.	Observed.	Calculated.
1	3·5	3·4
2	7·0	6·8
3	10·3	10·2
4	15·0	13·6
5	17·5	17·0
6	20·5	20·4
7	24·0	23·8
8	26·3	27·2
9	30·0	30·6
10	32·3	34·0

By mercurial gauge.

Tension.	Deflection.	Absorption.
½ inch	48°·5	60
1 inch	56·5	96

Table X.—Iodide of Amyle.

Unit-measure $\frac{1}{10}$th of a cubic inch.

	Absorption.	
Measures.	Observed.	Calculated.
1	0·6	0·57
2	1·0	1·1
3	1·4	1·7
4	2·0	2·3
5	3·0	2·9
6	3·8	3·4
7	4·5	4·0
8	5·0	4·6
9	5·0	5·1
10	5·8	5·7

The deflections here are very small; the substance, however, possesses so feeble a volatility, that the tension of a measure of its vapour, when diffused through the experimental tube, must be infinitesimal. With the specimen which I examined, it was not practicable to obtain a tension sufficient to depress the mercury gauge ½ an inch; hence no observations of this kind are recorded.

Table XI.—Chloride of Amyle.

Unit-measure $\frac{1}{10}$th of a cubic inch.

	Absorption.	
Measures.	Observed.	Calculated.
1	1·3	1·3
2	3·0	2·6
3	3·8	3·9
4	5·1	5·2
5	6·8	6·5
6	8·5	7·8
7	9·0	9·1
8	10·9	10·4
9	11·3	11·7
10	12·3	13·0

By mercurial gauge.

Tension.	Deflection.	Absorption.
½ inch	59°	137
1 inch	not practicable.	

Table XII.—Benzole.

Unit-measure $\frac{1}{10}$th of a cubic inch.

Measures.	Absorption. Observed.	Calculated.
1	4·5	4·5
2	9·5	9·0
3	14·0	13·5
4	18·5	18·0
5	22·5	22·5
6	27·5	27·0
7	31·6	31·5
8	35·5	36·0
9	39·0	40·0
10	44·0	45·0
11	47·0	49·0
12	49·0	54·0
13	51·0	58·5
14	54·0	63·0
15	56·0	67·5
16	59·0	72·0
17	63·0	76·5
18	67·0	81·0
19	69·0	85·5
20	72·0	90·0

Up to the 10th measure, or thereabouts, the proportion between density and absorption holds good, from which onwards the deviation from the law gradually augments.

By mercurial gauge.

Tension.	Deflection.	Absorption.
$\frac{1}{2}$ inch	54	78
1 inch	57	103

Table XIII.—Methylic Alcohol.

Unit-measure $\frac{1}{10}$th of a cubic inch.

Measures.	Absorption. Observed.	Calculated.
1	10·0	10·0
2	20·0	20·0
3	30·0	30·0
4	40·5	40·0
5	49·0	50·0
6	53·5	60·0
7	59·2	70·0
8	71·5	80·0
9	78·0	90·0
10	84·0	100·0

By mercurial gauge.

Tension.	Deflection.	Absorption.
½ inch	58°·8	133
1 inch	60·5	168

TABLE XIV.—Formic Ether.

Unit-measure $\frac{1}{10}$th of a cubic inch.

	Absorption.	
Measures.	Observed.	Calculated.
1	8·0	7·5
2	16·0	15·0
3	22·5	22·5
4	30·0	30·0
5	35·2	37·5
6	39·5	45·0
7	45·0	52·5
8	48·0	60·0
9	50·2	67·5
10	53·5	75·0

By mercurial gauge.

Tension.	Deflection.	Absorption.
½ inch	58°·8	133
1 inch	62·5	193

TABLE XV.—Propionate of Ethyle.

Unit-measure $\frac{1}{10}$th of a cubic inch.

	Absorption.	
Measures.	Observed.	Calculated.
1	7·0	7·0
2	14·0	14·0
3	21·8	21·0
4	28·8	28·0
5	34·4	35·0
6	38·8	42·0
7	41·0	49·0
8	42·5	56·0
9	44·8	63·0
10	46·5	70·0

By mercurial gauge.

Tension.	Deflection.	Absorption.
½ inch	60·5	168
1 inch	not practicable.	

Table XVI.—Chloroform.

Unit-measure $\frac{1}{10}$th of a cubic inch.

Measures.	Absorption. Observed.	Calculated.
1	4·5	4·5
2	9·0	9·0
3	13·8	13·5
4	18·2	18·0
5	22·3	22·5
6	27·0	27·0
7	31·2	31·5
8	35·0	36·0
9	39·0	40·5
10	40·0	45·0

Subsequent observations lead me to believe that the absorption by chloroform is a little higher than that given in the above Table.

Table XVII.—Alcohol.

Unit-measure $\frac{1}{2}$ a cubic inch.

Measures.	Absorption. Observed.	Calculated.
1	4·0	4·0
2	7·2	8·0
3	10·5	12·0
4	14·0	16·0
5	19·0	20·0
6	23·0	24·0
7	28·5	28·0
8	32·0	32·0
9	37·5	36·0
10	41·5	40·0
11	45·8	44·0
12	48·0	48·0
13	50·4	52·0
14	53·5	56·0
15	55·8	60·0

By mercurial gauge.

Tension.	Deflection.	Absorption.
$\frac{1}{2}$ inch	60	157
1 inch	not practicable.	

The difference between the measurements when equal *tensions* and when equal *volumes* at the maximum density are made use of is here strikingly exhibited.

In the case of alcohol I was obliged to resort to a unit-measure of $\frac{1}{2}$ a cubic inch to obtain an effect about equal to that

produced by benzole with a measure possessing only $\frac{1}{10}$th of a cubic inch in capacity; and yet for equal tensions of 0·5 of an inch, alcohol cuts off precisely twice as much heat as benzole. There is also an enormous difference between alcohol and sulphuric ether when equal measures at the maximum density are compared; but to bring the alcohol and ether vapours up to a common tension, the density of the former must be many times augmented. Hence it follows that when *equal tensions* of these two substances are compared, the difference between them diminishes considerably. Similar observations apply to many of the substances whose deportment is recorded in the foregoing Tables; to the iodide and chloride of amyle, for example, and to the propionate of ethyle. Indeed it is not unlikely that with equal tensions the vapour of a perfectly pure specimen of the substance last mentioned would be found to possess a higher absorptive power than that of ether itself.

It has been already stated that the tube made use of in these experiments was of brass polished within, for the purpose of bringing into clearer light the action of the feebler gases and vapours. Once, however, I wished to try the effect of chlorine, and with this view admitted a quantity of the gas into the experimental tube. The needle was deflected with prompt energy; but on pumping out *, it refused to return to zero. To cleanse the tube, dry air was introduced into it ten times in succession; but the needle pointed persistently to the 40th degree from zero. The cause of this was easily surmised: the chlorine had attacked the metal and partially destroyed its reflecting power; thus the absorption by the sides of the tube itself cut off an amount of heat competent to produce the deflection mentioned above. For subsequent experiments the interior of the tube had to be re-polished.

Though no other vapour with which I had experimented produced a permanent effect of this kind, it was necessary to be perfectly satisfied that this source of error had not vitiated the experiments. To check the results, therefore, I had a length of 2 feet of similar brass tube coated carefully on the inside with lampblack, and determined by means of it the absorptions of all the vapours which I had previously examined, at a common tension of 0·3 of an inch. A general corroboration was all I sought, and I am satisfied that the few discrepancies which the measurements exhibit would disappear, or be accounted for, in a more careful examination.

In the following Table the results obtained with the blackened and with the bright tubes are placed side by side, the tension

* Dense dark fumes rose from the cylinders on this occasion; a similar effect was produced by sulphuretted hydrogen.

in the former being three-tenths, and in the latter five-tenths of an inch.

TABLE XVIII.

Vapour.	Absorption. Bright tube, 0·5 tension.	Blackened tube, 0·3 tension.	Absorption with bright tube proportional to
Bisulphide of Carbon . .	5·0	21	23
Iodide of Methyle . . .	15·8	60	71
Benzole	17·5	78	79
Chloroform	17·5	89	79
Iodide of Ethyle . . .	21·5	94	97
Wood-spirit	26·5	123	120
Methylic Alcohol . . .	29·0	133	131
Chloride of Amyle . . .	30·0	137	135
Amylene	31·8	157	143

The order of absorption is here shown to be the same in both tubes, and the quantity absorbed in the bright tube is, in general, about $4\frac{1}{2}$ times that absorbed in the black one. In the third column, indeed, I have placed the products of the numbers contained in the first column by 4·5. These results completely dissipate the suspicion that the effects observed with the bright tube could be due to a change of the reflecting power of its inner surface by the contact of the vapours.

With the blackened tube the order of absorption of the following substances, commencing with the lowest, stood thus:—

Alcohol,
Sulphuric ether,
Formic ether,
Propionate of ethyle;

whereas with the bright tube they stood thus:—

Formic ether,
Alcohol,
Propionate of ethyle,
Sulphuric ether.

As already stated, these differences would in all probability disappear, or be accounted for on re-examination. Indeed very slight differences in the purity of the specimens used would be more than sufficient to produce the observed differences of absorption*.

[To be continued.]

* In illustration of this I may state, that of two specimens of methylic alcohol with which I was furnished by two of my chemical friends, one gave an absorption of 84 and the other of 203. The former specimen had been purified with great care, but the latter was not pure. Both specimens, however, went under the common name of methylic alcohol. I have had a special apparatus constructed with a view to examine the influence of ozone on the interior of the experimental tube.

XXIV. *Some Remarks on* Dr. Siemens's *Paper "On Standards of Electrical Resistance, and on the Influence of Temperature on the Resistance of Metals."* *By* A. Matthiessen, *F.R.S.**

1. In the above paper †, page 92 (2nd paper), M. Siemens states, "*It may be asserted without all doubt, that the most experienced and skilful physicists, even with the best instruments and most appropriate localities, are not able to determine resistances in absolute measure which do not vary several per cent. A standard of so little accuracy would not even answer the requirements of technical purposes.*" M. Siemens, however, does not give the grounds on which he bases the above assertion.

Prof. W. Thomson, in a paper published in the 'Proceedings of the Royal Society' (vol. viii. p. 555), says, "*It is impossible to over-estimate the great practical value of this system of absolute measurement carried out by Weber into every department of electrical science.*" I have always understood that the determinations of resistances in absolute measure by Weber's methods were most accurate; and in order to be able to answer this point more definitely, I wrote to Prof. W. Thomson and asked him to give me his opinion on the subject, knowing that the opinion of such a distinguished physicist would have great weight—in fact, would settle the question.

Prof. Thomson's answer was the following:—

"*There can scarcely be a doubt but that Weber's original determination of resistance in absolute measure* (Pogg. Ann. vol. lxxxii. p. 33) *was considerably within one half per cent. of the truth. He used two remarkably different methods, and obtained by means of them* 190·3 and 189·8 *respectively for the absolute measure of the resistance of one of his conductors. The details of the application of each of the two methods separately present so much consistence, that the possibility of so great an error as one half per cent. could not be admitted in the mean result of either considered alone, unless through some error in the corrections directly applied to it. Any such doubt seems perfectly removed by the close agreement between the two results derived from the two different methods, with different instruments, very dissimilar experimental operations, and perfectly distinct reductions and corrections to reduce to absolute measure. The mean of the two numbers quoted above, being* 190·05, *differs by less than* 0·1 *per cent. from each of them. It is not improbable that this mean may be within* 0·1 *per cent. of the truth: it is*

* Communicated by the Author.

† Pogg. *Ann.* vol. cxiii. p. 91. In order to prevent mistakes I will call this the 2nd paper; the 1st paper being the original one, where M. Siemens proposes mercury as a standard (Phil. Mag. Jan. 1861).

improbable that it differs by 0·2 *per cent. from the truth, and it is scarcely possible that it is wrong by* 0·5 *per cent.**"

2. M. Siemens states, page 93 (2nd paper), "Because the differences found in the conducting powers of the gold-silver alloys I had made in different places amount to 1·5 per cent., the alloy is useless for the purpose proposed by me (Phil. Mag. Feb. 1861), namely, the reproduction of a resistance by means of which the observations of different experimenters may be compared with each other, or the reproduction of a resistance in absolute measure. For if two alloys are made and their resistances determined, we should certainly come within one half per cent. of the true value, six out of the eight alloys tested agreeing within that limit." Let us now for a moment see what M. Siemens says of his proposed mercury standard; and on referring to his first paper we find a Table, where he gives the resistances of six tubes filled with mercury. The values found by him for $\frac{w}{w_1}$, where w is the calculated and w_1 the observed resistances, are given in the following Table, together with those found by myself for the conducting power of the gold-silver alloy.

Table I.

No. of tube.	Values found for $\frac{w}{w_1}$.	Conducting power of hard-drawn alloys.
1	1·008	1 = 1·003
2	1·000	2 = 1·002
3	1·0008	3 = 0·988
4	0·992	5 = 1·004
5	0·994	6 = 0·997
6	1·005	7 = 1·003
		8 = 1·001

M. Siemens, when speaking of the differences he found, says *they are not greater than were to be expected*; and further on, he continues, *the temperatures of the* étalon (*copper*) *and the mercury varied* 2—3° C. *during the experiments*; but does not state which of the determinations were made at the higher or lower temperature, so that the differences he finds may be greater or smaller, as the case may be. Now on comparing the above values it will be seen that the maximum differences are in each case the same. If, therefore, in the opinion of M. Siemens, the gold-silver alloy is useless as a standard, how much more must his mercury standard be so, when, according to his own determinations with

* Prof. Weber, in a letter written a short time since to Prof. Thomson, states, when speaking of some new determinations of resistance in absolute measure he is about to undertake, that by some improvements in the method and apparatus he hopes to arrive *at a still greater accuracy* than that which he formerly obtained.

the same mercury in tubes carefully picked from a large quantity, he does not arrive at a greater accuracy than I did with alloys made in *different* places, by *different* persons, of *different* gold and silver, and drawn by *different* wire-drawers. If, on the contrary, I had made and drawn the eight alloys myself of the same gold and silver, I should undoubtedly have obtained results not varying 0·1 per cent. If now different experimenters determine the conducting power of mercury, is it not probable that much greater differences would be found between their results than those obtained by M. Siemens *himself*? Now it so happens that different observers have already determined the conducting power of mercury. Let us compare their results; and we will first compare the conducting powers of the metals, taking silver =100, and afterwards taking mercury =100. Now I maintain that if the values obtained for one and the same metal by these different observers agree better when compared with silver than with mercury as unit, then M. Siemens's proposed standard must be useless as such.

TABLE II.—Conducting Power of Metals. Silver = 100.

	Siemens.	Lenz.	Becquerel.	Matthiessen.
Silver*	100	100	100	100
Copper*.....	96·9†	73·4	95·3	99·5
Gold*.....	.....	58·5	68·9	78
Cadmium ...	.....	.....	26·3	23·8
Zinc	.....	.....	25·7	29·2
Tin	.....	22·6	15·0	12·3
Iron	.....	13·0	13·1	14·4 at 20·4
Lead	.....	10·7	8·8	8·3
Platinum* ...	14·2	10·4	8·6	10·5 at 20·7
Mercury.....	1·72	3·42 at 18·9	1·86	1·65

TABLE III.—Conducting Power of Metals. Mercury = 100.

	Siemens.	Lenz.	Becquerel.	Matthiessen.
Silver*	5820	2924	5376	6060
Copper*.....	5640	2146	5123	6030
Gold*.....	.....	1710	3704	4727
Cadmium ...	.....	.....	1414	1442
Zinc	.....	.....	1382	1770
Tin	.....	659	810	745
Iron	.....	380	704	872 at 20·4
Lead	.....	312	473	503
Platinum* ...	825	304	462	636 at 20·7
Mercury.....	100	100 at 18·7	100	100

* Hard-drawn. All temperatures 0° C., except when the contrary is stated.

† Value given in 2nd paper 100.

One glance at the foregoing Table will suffice to show how very badly Lenz's series agrees with the rest when mercury is taken as unit; and comparing Becquerel's and my own, our values differ for—

Table IV.

	When silver = 100.	When mercury = 100.
Copper	4·3 per cent.	15 per cent.
Gold	11·6 „	21·6 „
Cadmium . . .	9·0 „	2·0 „
Zinc	11·9 „	22·1 „
Tin	18·0 „	8·0 „
Iron	9·0 „	19·2 „
Lead	5·7 „	5·9 „
Platinum	18·1 „	27·3 „

These results prove that the mercury standard proposed by M. Siemens cannot be a useful and good one; for, in fact, we obtain more concordant results if we take in the above series any other metal as unit. The mercury employed by three of the observers was stated by them to have been pure.

3. Page 93 (2nd paper), M. Siemens states, "*German-silver wire is much better for resistance coils than the gold-silver alloy, on account of its high conducting power and expense.*" I quite agree with him. I only proposed the gold-silver alloy to be used for the same purpose as he does mercury.

4. Page 93 (2nd paper), M. Siemens states, "*Even if the conducting power of the gold-silver alloys were the same, yet small resistances cannot be accurately compared with them, as there would always be a slight difference in the resistance at points where the alloy is connected with the connectors of the apparatus.*" I may, however, mention that I always solder the ends of the normal wire to two thick copper wires (of 2–3 millims. diameter and about 35 millims. long), the free ends of which are carefully amalgamated by dipping them into a solution of nitrate of mercury in dilute nitric acid; and the connexions are made by means of mercury cups, the bottoms of which are amalgamated copper plates. These can be removed, and are of course from time to time reamalgamated. The free copper ends of the normal wire are reamalgamated every time before use. This arrangement gives most satisfactory results; not the slightest change in the resistance is observed when the normal wire is taken out of the mercury cups and put in again. If, however, a wire of the gold-silver alloy has once been made and arranged for use, when wanted it is only necessary to reamalgamate the ends, and it may then be used without further loss of time. On the contrary, for M. Siemens's proposed unit there must be a great deal of time

spent in cleaning the tube (in which operation the tube is liable to be broken) and in purifying the mercury.

5. Page 95 (2nd paper), M. Siemens gives a table, by which he wishes to prove that he is able to reproduce resistances of exactly the same values. He, however, only proves that he is able to fill the same tubes with different mercury, and that their resistances only vary 0·05 per cent.; for he compared three unknown resistances with two equal ones (when reduced to equal lengths and diameters), and obtained very nearly the same values. Now if, instead of taking normal tubes, called 3 and 7, he had taken those called No. 1 and 4 (1st paper), would his results have been the same? No; they would have varied 1·5 per cent. (See his results given in Table I.)

6. Page 96 (2nd paper), M. Siemens says, the statement I made *that the traces of foreign metals cause a decrement in the conducting power of mercury, and not, as stated by* Siemens, *an increment, is incorrect.* In this M. Siemens is perfectly right. I was misled by the fact that when mercury is alloyed with several per cent. of foreign metal, a smaller conducting power is observed than the mean of the conducting powers of the relative volumes of the metals employed; and as in no case I had found an increment in the conducting power of a metal when alloyed with a trace of another, I concluded that traces (0·1 or 0·2 per cent.) of foreign metals would also cause a decrement in the conducting power of mercury.

As mercury behaves in this respect differently from the other metals, instead of assuming, as I did in my paper on the conducting power of alloys*, that the metals may be classed under two heads, viz.,—

I. Those metals which, when alloyed with each other, conduct electricity in the ratio of their relative volumes;

II. Those metals which, when alloyed with one of the metals belonging to the first class or with one another, *do not* conduct electricity in the ratio of their relative volumes, but *always* in a lower degree than the mean of their volumes,—

we must now have three classes of metals, the third probably being—

Those metals which, when alloyed with very small per-centages of another, have a *greater* conducting power, but when alloyed with larger per-centages, have a *lower* conducting power than the mean of their volumes. I am at present investigating how far this may be true; and it will be very interesting to see whether pure metals, such as bismuth, tin, &c., in a liquid state behave like mercury; that is to say, if, when melted, traces of other metals be added, an increment in the conductor will be observed. I also

* Phil. Trans. 1860, p. 161.

intend trying whether the conducting power of mercury when solid is increased or decreased by the addition of traces of other metals.

To prove the assumption I have made as to the behaviour of the third class of metals is probably correct, I have given in Table V. some experiments.

Table V.

Taking the conducting power of the hard-drawn gold-silver alloys at 0° = 100,—

			Calculated conducting power.
Pure mercury conducts	24·47 at 18° C.		
,, alloyed with 0·1 per cent. pure bismuth	24·58 at 18·6	24·46
,, ,, 0·01 per cent. pure tin	24·51 at 18·4	24·50
,, ,, 0·02 ,,	24·54 at 18·0	24·52
,, ,, 0·05 ,,	24·63 at 18·2	24·61
,, ,, 0·1 ,,	24·76 at 18·8	24·75
,, ,, 0·2 ,,	25·04 at 19·0	25·02
,, ,, 0·5 ,,	25·86 at 18·4	25·83
,, ,, 1·0 ,,	26·62 at 18·6	27·19
,, ,, 2·0 ,,	27·66 at 18·8	29·19
,, ,, 4·0 ,,	29·69 at 19·0	35·09

For the calculations, the conducting power of tin was taken at 172·09, that of bismuth 17·88; the specific gravity of mercury 13·573, that of bismuth 9·823, and that of tin 7·294.

The resistances of the amalgams were determined in the same tube as the mercury, so that any error in the measurement of the length or diameter will not have any influence on the relative values obtained.

From the above Table we see that even bismuth, a worse conductor than mercury, increases the conducting power of mercury, as would be expected from the above assumption. The experiments with the amalgams show how important it would be, if mercury were to be taken as unit for determinations of resistances, that it should be *absolutely chemically pure*. We cannot be surprised to find discrepancies in the values obtained for mercury by different observers, when such small traces of impurity so materially affect its conducting power.

7. Page 103 (2nd paper), M. Siemens gives a Table, from which he deduces that the increase in the resistance of mercury between 0° and 100° C. is in direct ratio with the increase of temperature. In other words, M. Siemens assumes that the formula

$$w = 1 + at$$

expresses the resistance of mercury at any temperature between 0° and 100°. Let us now calculate from his results the values

of "*a*," the temperatures at which the resistances have been observed. These will be found by using the formula $a=\frac{w-1}{t}$.

In Table VI. I have given M. Siemens's Table of the resistances of mercury for different temperatures, together with the value of the coefficient "*a*" for each of the observations. The resistance of mercury at 0° C. is taken = 1.

Table VI.

T.	Resistance.	Coefficient "*a*"
0	1·00	
18·51	1·0166	0·000897
28·19	1·0263	0·000933
41·29	1·0391	0·000947
57·34	1·0548	0·000956
97·29	1·0959	0·000986

If the formula $w=1+at$ were correct, the values found for "*a*" ought all to be equal; but as there is a gradual increment in the values there can be no doubt that a formula with two terms, as $w=1+at+bt^2$, will express the resistances for different temperatures much better.

The increase of resistance of mercury between 0° and 100° is, according to

Becquerel.	Siemens.	Matthiessen and von Bose.	Schröder van der Kolk*.
10·3 per cent.	9·85 per cent.	9·0 per cent.	8·6 per cent.

M. Siemens's value is deduced from 12 observations; Schröder van der Kolk's from 29; von Bose's and my own from 96. Again, M. Siemens deduces from 14 observations that the resistance of copper between 0° and 100° increases in direct ratio with the increase of temperature; whereas von Bose and myself deduce from 332 observations that the formula for the resistance of copper must be $w=1+at+bt^2$. Our experiments are almost finished; we hope that they will be published before the end of the year.

8. Page 105 (2nd paper), M. Siemens states, "*What induced Mr. Matthiessen to make at the end of his paper the following assertion I am not able to judge, as he does not give the grounds on which he bases it*:—'*it has been generally assumed that the conducting power of all copper wire, whether pure or commercial, varies with an increase of temperature to the same degree, which, however, is far from the truth.*'" Two reasons for my having made the above

* Poggendorff's *Annalen*, vol. cx. p. 452.

assertion were, (1) M. Siemens himself assumes in his first paper that the conducting power of his copper (étalon) varies 0·1 per cent. with each degree Centigrade; and (2) M. C. W. Siemens*, in describing his resistance thermometer, assumes also the same; in fact he bases his calculation on Arndtsen's formula without stating the sort of copper he uses.

That my statement regarding the difference of the coefficients of the increase of resistance for different temperatures of coppers is correct, may be deduced from the following data:—M. Siemens finds (2nd paper) the resistances of a commercial copper he tested to vary between 0° and 100° C. 32·9 per cent.; Arndtsen finds copper containing traces of iron to vary 36 per cent.; von Bose and myself have found pure copper to vary 42 per cent.; and lastly, one commercial copper I have tested varies only about 8 per cent.

XXV. *On the True and False Discharge of a Coiled Electric Cable.* *By* Professor W. Thomson, *LL.D., F.R.S., and* Mr. Fleeming Jenkin, *C.E.*†

IN an article in the last May Number of this Magazine, "On the Galvanic Polarization of buried Metal Plates," translated from Poggendorff's *Annalen*, No. 10, 1860, Dr. Carl describes certain interesting experiments on the electro-polarization produced between two large zinc plates buried in the garden of the Observatory of Munich, by opposing and by augmenting the natural earth-current between them by the application of a single element of Daniell's; and concludes with the following remark:—

"The above experiments disclose nothing at variance with the known laws of galvanism; but it nevertheless appeared to me advisable to make them known, as they afford a simple explanation of certain phenomena which Professor Thomson has described (Report of the Twenty-ninth Meeting of the British Association, Aberdeen, 1859, Trans. of Sections, p. 26), and which he seems to attribute to entirely different causes."

In the report of Prof. Thomson's communication to the British Association here referred to, it is stated that (after mentioning certain experiments by Mr. F. Jenkin on submarine cables coiled in the manufactory of Messrs. Newall and Co., Birkenhead, in which one end of the battery used, and one end of the cable experimented on, in each case was kept in connexion with the earth while the other end of the cable, after having been for a time in

* Phil. Mag. January 1861.

† Communicated by the Authors.

connexion with the insulated pole of the battery, was suddenly removed from the battery and put in connexion with the earth through the coil of a galvanometer) Prof. Thomson and Mr. Fleeming Jenkin remarked "that the deflections recorded in these

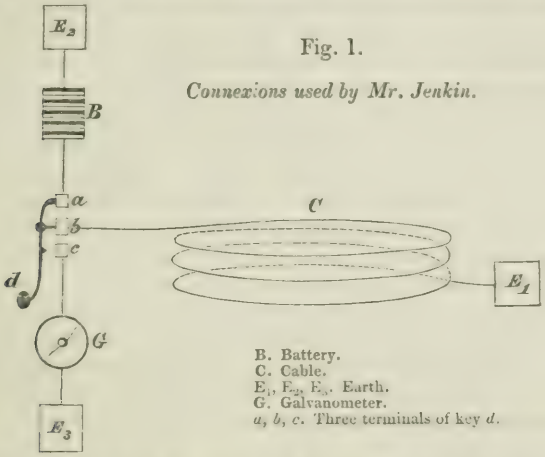

Fig. 1.

Connexions used by Mr. Jenkin.

B. Battery.
C. Cable.
E_1, E_2, E_3. Earth.
G. Galvanometer.
a, *b*, *c*. Three terminals of key *d*.

experiments were in the contrary direction to that which the true discharge of the cable would give;" and at Prof. Thomson's request "Mr. Jenkin repeated the experiments, watching carefully for indications of reverse currents to those previously noted. It was thus found that the first effect of pressing down the key [to throw the cable from battery to earth through galvanometer] was to give the galvanometer a deflection in the direction corresponding to the true discharge current, and that this was quickly followed by a reverse current generally greater in degree, which gave a deflection corresponding to a current in the same direction as that of the original flow through the cable.

"Professor Thomson explained this second current, or false discharge, as it has since been sometimes called, by attributing it to mutual electro-magnetic induction between different parts of the coil, and anticipated that no such reversal could ever be found in a submerged cable. The effect of this induction is to produce in those parts of the coil first influenced by the motion of the key, a tendency for the electricity to flow in the same direction as that of the decreasing current flowing through the remoter parts of the coil. Thus, after the first violence of the back flow through the key and galvanometer, the remote parts of the cable begin, by their electro-magnetic induction on the near parts, to draw electricity back from the earth through the galvanometer into the cable again, and the current is once more in one and the same direction throughout the cable."

The phenomena thus described and explained are entirely different from any that could result from the galvanic polarization supposed by Dr. Carl to account for them*. It is true that the discharging earth-plate might become polarized by the discharge in certain cases sufficiently to cause a slight reversal in the current through the galvanometer coil, after the subsidence of the violent discharge current through it. But in no case could the whole quantity of electricity flowing in this supposed polarization current be more than a very small fraction of the quantity which previously flowed in the true discharge current, of which it is a feeble electro-chemical reflexion. Its effect on the galvanometer needle must in every case be as nothing in comparison to the great impulsive deflection produced by the true discharge current; and there is no combination of circumstances, as to size of the earth-plates, amount of the battery power, and rapidity or sensibility of the galvanometer needle, in which the cause supposed by Dr. Carl could possibly be adequate to explain the phenomena described in Prof. Thomson's communication.

In point of fact, all effects of polarization of the earth-plates were extremely small in comparison with the main currents observed, which in the experiments on cables with one end kept to earth, consisted of (1) the constant *through-current*, produced by a battery of 72 elements Daniell's in series; (2) the true discharge through the galvanometer to be observed instantly after breaking the battery connexion of the end of the cable to which the battery was applied, and making instead a connexion, through the galvanometer coil, between the same end of the cable and the earth; and (3) the "false discharge," so called because it must have been often mistaken for the true discharge, which almost necessarily escapes notice altogether when short lengths of coiled cable are tested with slow galvanometer nee-

* They are also different from any effects which could result from polarization of the plate connecting the far end of the cable with earth—a cause suggested by Prof. Wheatstone in a report published by the Committee appointed by the Board of Trade to inquire into the Construction of Submarine Cables. In support of his opinion, Prof. Wheatstone quotes some experiments in which he could observe only the well-known effects due to polarization, which on the short pieces of wire at his command quite overpowered both the true and false discharge. The current from the polarized end of a cable is always in the direction of the true discharge when the battery has been long enough applied: it is observed on both straight and coiled cables, and is capriciously variable. The details given in the present paper show that the currents due to electro-magnetic induction, called false discharge currents, are on the contrary always in the opposite direction to that of the true discharge, that they can only be observed on coiled cables, and that they are in each case sensibly constant. The galvanometer used by Mr. Jenkin would not have been deflected half a degree by the current from a polarized earth-plate at the end of cables from 300 to 500 knots in length.

dles. The *through-current* (1) was measured at the beginning of the discharge experiments by introducing the galvanometer into the circuit of cable and battery. Neither the whole amount of the true and false discharges, nor the rapidly varying strength of the current from instant to instant, could be distinctly observed, because the period of vibration of the galvanometer needle, being about $4\frac{1}{2}$ seconds each way, was neither incomparably greater nor incomparably smaller than the duration of the current in either direction. Thus the back-flow, or true discharge, which was of comparatively short duration, first gave the needle an impulse to the left (let us suppose); but before its natural swing, from even an instantaneous impulse, could have allowed it to begin to return, it was caught by the reverse current of false discharge and turned and thrown to the other side of zero through an angle to the right, which, except in the cases of the longest lengths of cable experimented on, was much greater than the angle of the first deflection to the left. It is obvious from what has been stated, that the durations of these deflections of the needle on the two sides do not even approximately coincide with the times during which the current flowed in the directions of the true and false discharges respectively, but that they depend in a complicated manner on the inertia of the needle and the varying forces to which it is subjected. The general character of the phenomena will be made sufficiently clear by the following examples, which are quoted from letters of Mr. Jenkin's to Prof. Thomson, of dates April 9 and April 22, 1859.

Table I.

Lengths of cable in nautical miles*,—the first being for the Dardanelles, and the other three, of a different gauge, for the Alexandria and Candia telegraph.	Remote end of cable kept insulated.	Remote end of cable kept to earth.
	First throw of needle.	First observed throw of needle.
123	12° left	32° right
$137\frac{3}{4}$	$15\frac{1}{2}$ „	37 „
$261\frac{1}{2}$	$28\frac{1}{2}$ „	31 „
$399\frac{1}{4}$	$41\frac{1}{2}$ „	21 „

To explain the cause of the deflections to the right recorded in the last column of this Table, the following observations were made, with care that the first motion of the needle in either direction, however slight or rapid, should not escape notice.

* A nautical or geographical mile, or a knot as it is generally called in nautical language, is taken as 6087 feet.

TABLE II.—455 nautical miles of Alexandria and Candia Cable.

Remote end of cable kept	First throw of needle.	Recoil or second throw.	Excess of recoil above first throw.
1. To earth direct	$2\frac{1}{2}^\circ$ right	$24\frac{1}{2}^\circ$ left	22°
2. To earth through 10 German miles, resistance units*	5 „	22 „	17
3. To earth through 50 „	$11\frac{1}{2}$ „	$18\frac{1}{2}$ „	7
4. To earth through 90 „	$16\frac{1}{2}$ „	21 „	$4\frac{1}{2}$
5. Insulated	$44\frac{1}{2}$ „	not observed	
6. To earth direct, and key "pressed very sharp home"	$3\frac{1}{4}$ „	24 „	$20\frac{3}{4}$

If the whole duration of current, with or without reversal, through the galvanometer coil had been infinitely small in comparison with the natural time of oscillation of the needle (which, reckoned in one direction, was about $4\frac{1}{2}$ seconds), the recoils would have been sensibly equal to the first throws in the contrary direction, being only less by the effect of resistance of the air, &c. to the motion of the needle. Hence the numbers in the last column of the preceding Table prove that at some interval of time, not incomparably less than $4\frac{1}{2}$ seconds, after the first motion of the needle, there was a current through the galvanometer coil opposite in direction to that which produced the first or *right* deflection, in each case except No. 5, or that in which the remote end of the cable was insulated. It may be safely assumed that the conductors used in cases 2, 3, and 4 to give the stated resistances between the remote end of the cable and the earth, exercised no sensible electro-magnetic influence, and held no sensible charge, in the actual circumstances; and it is interesting to see how the greater the resistance thus introduced, that is to say the more nearly the remote end is insulated, the greater is the first throw (due, as explained above, to true discharge), and the less is the excess of the recoil above it.

This excess, shown in the last column of the Table, exhibits the effect of the electro-magnetic induction from coil to coil which stops short the true discharge, and produces after it a reverse current constituting the "false discharge." The following experiments, performed by Mr. Jenkin on the 19th of April, 1859, on different lengths of the Red Sea cable, illustrate the relations between true and false discharge.

* The resistance of this unit was found by experiment to be equal to about 190×10^6 British absolute units of feet per second, or to $6\frac{1}{4}$ nautical miles of the Alexandria and Candia cable, or to 4·39 of the Dardanelles, or to 7·44 of the Red Sea.

TABLE III.

Lengths of Red Sea cable.	Remote end of length used kept insulated. Discharge from electrification of 36 cells.			Remote end of length used kept to earth. True and false discharge from electrification and current of 72 cells.		
	First throw.	Recoil.	Excess of first throw above recoil.	First throw.	Recoil.	Excess of recoil above first throw.
312 nautical miles	20° left	19° right	1°	1⅓° left	18° right	16¾°
546 „	29½° „	27 „	2½°	5¾° „	15 „	9¼°
858 „	35 „	14 „	21	17 „	22 „	5
Col. 1.	Col. 2. True discharge.	Col. 3. Inertia of needle.	Col. 4. Effect of duration of discharge.	Col. 5. True discharge.	Col. 6. "False discharge" and inertia.	Col. 7. "False discharge," or effect of electro-magnetic induction.

The great increase of the numbers in column 4, for the longer portions of cable, illustrates the fact first demonstrated by Prof. Thomson in 1854*, that, when undisturbed by electro-magnetic induction, the discharge of a cable takes place at a rate inversely proportional to the square of the length. The duration of the discharge, which, when the remote end is kept insulated, is probably much increased by electro-magnetic induction, must be very considerable in the case of the 858 miles length, to produce so great a diminution as 21° in the recoil, from a throw of 35°, on a needle whose period of vibration was 4½ seconds. The diminution of 1° from the throw of 20°, as observed in the case of the 312 miles length, may be to some considerable proportion of its amount due to resistance of the air, although, as this is probably scarcely sensible on a single swing of the needle, it may be supposed that it is chiefly the effect of the duration of the discharge current. From column 7 it is clear that nearly all trace of the electro-magnetic influence would be lost sight of in comparison with the greater effect of true discharge, in the method of experimenting that was followed, if applied to lengths exceeding 1000 knots, in a coil or coils of similar dimensions to those actually used; while for the 546 knots, and shorter lengths, the effect of electro-magnetic induction is greater than that of the true discharge. It is remarkable that the effect of electro-magnetic induction is absolutely greatest for the shortest of the three lengths. These relations between the different lengths must of course, according to the explanation we have given, depend on the plan of coiling, whether in one coil or in several coils, and on the dimensions of the coil or coils, as

* Proceedings of the Royal Society, 1855; and Phil. Mag. vol. xi. p. 146.

well as on the dimensions of the conductor, the gutta percha, and the outer iron sheath of the cable. The magnetic properties of the iron sheath must greatly influence the false discharge; and it would be interesting to compare the discharge from a plain gutta-percha-covered wire coiled under water with that from an iron-sheathed cable.

The following set of experiments, the last which we at present adduce, illustrate the influence of less or greater intervals of time during which the near end of the cable remains insulated, after removal from the battery but before application to earth through the galvanometer coil.

TABLE IV.—455 nautical miles of Alexandria and Candia Cable, remote end kept to earth. Battery of 72 cells Daniell's.

Experiment.		Throw of needle by true discharge.	Recoil, if any, and throw by false discharge.
No.			
1	Key struck down	3° left	27° right
2	Key pressed down as usual .	2¾ „	26 „
3	Key pressed very gently ...	2½ „	20½ „
4	Key held 5 seconds half-way.	0 „	14 „
5	„ 10 „ „	0 „	17 „
6	„ 15 „ „	0 „	4 „

In order to detect whether there might not have been "a slight hesitation in these three last instances, a much more delicate instrument was taken, but no such hesitation could be detected." These results are very remarkable, especially as regards the duration of the electro-magnetic influence. If the conductor of the cable were circumstanced like that of a common electro-magnet, and had no sensible electrostatic capacity, the "mechanical value* of the current in it" at the instant of the connexion between its near end and the battery being broken, would be spent in a spark, or electric arc of sensible duration between the separated metal surfaces. But in the cable, the electrostatic capa-

* See a paper "On Transient Electric Currents," by Prof. W. Thomson, Phil. Mag. June 1853, where it is shown that, like the mechanical value of the motion of a moving body, which is equal to half the square of its velocity, multiplied by its mass, the mechanical value of a current at any instant, in a coiled conductor, depending on electro-magnetic induction, is equal to half the square of the strength of the current through it, multiplied by a constant which the author defined as the "electrodynamic capacity of the conductor," and which he showed how to calculate according to the form and dimensions of the coil. Additional explanations and illustrations will be found in Nichol's 'Cyclopædia of Physical Science,' second edition, 1859, under the heads "Magnetism—Dynamical Relations of," and "Electricity—Velocity of."

city of the near portions of the conductor has an effect analogous to that of Fizeau's condenser in the Ruhmkorff coils; and there was little or no spark (none was observed, although it was looked for, in the key) on breaking the battery circuit, and consequently, as nearly as may be, the whole mechanical value of the current left by the battery must have been expended in the development of heat in the conductor itself, and by induced currents in the iron of the sheath; and therefore we need not wonder at the great length of time during which electric motion remains in the cable.

The first column of results for experiments Nos. 1, 2, and 3, and the two columns for Nos. 4, 5, and 6, show that the continued flow of the main current through the cable, after the near end is removed from the battery and kept insulated, is to reduce its potential gradually from that of the battery (which for the moment we may call positive), through zero, to negative, in some time less than five seconds, and to keep it negative ever after, if it is kept insulated, as long as any trace of electro-dynamic action remains*. It is probable that, at the same time, there may be oscillations of current backwards and forwards again†, and of potential to negative, and positive again, in some parts, especially towards the middle, of the cable. The mathematical theory of the whole action is very easily reduced to equations; but anything like a complete practical analysis of these equations presents what may be safely called insuperable difficulties, because of the mutual electro-magnetic influence of the different parts of the cable with differently varying current through them. These peculiar difficulties do not, theoretically viewed, present any specially interesting features; and the problem is of little practical importance when once practical electricians are warned to avoid being misled by electro-magnetic induction, in testing by discharge during either the manufacture, the submergence, or lifting of a cable, and not to *under-estimate* the rate of signalling through a long submarine cable to be attained when it is laid, from trials through the same cable in coils, when electro-magnetic induction must embarrass the signalling more or less according to the dimensions and disposition of the coils, and probably does so in some cases to such an ex-

* After what has been said in the text above, it is scarcely necessary to point out that this effect is both opposed to, and much greater than, anything producible by polarization of the earth-plates.

† As in the oscillatory discharge of a Leyden phial, investigated mathematically by Prof. Thomson ("Transient Electric Currents," Phil. Mag. June 1853), and actually observed by Feddersen, in his beautiful photographic investigation of the electric spark (Poggendorff's *Ann.* vol. cviii. p. 497, probably year 1860; also second paper, year 1861).

tent as to necessitate a considerably slower rate of working than will be found practicable after the cable is laid.

The theoretical conclusion that the "false discharge" would not be observed in submerged cables, has been recently verified by Mr. Jenkin on various lengths of Bona cable up to 100 miles, which he was engaged in recovering, and which, under careful tests, never gave the slightest indication of "false discharge," although, even when the remote end had completely lost insulation, they gave not only polarization effects*, but also, in the same direction as these, but distinguishable from them, indications of true discharge. But, in fact, a fortnight before the theoretical conclusion was published by Prof. Thomson at the Aberdeen meeting, a most remarkable and decisive experimental demonstration of it was published by Mr. Webb, Engineer to the Electric and International Telegraph Company, who had independently discovered the phenomena which form the subject of this paper, and given substantially the same explanation as that which we now maintain. If there could be a doubt as to the electro-magnetic theory, the following extract from a letter of Mr. Webb's, published in 'The Engineer' of August 26, 1859, is decisive:—

"It is, however, on making contact at F with earth [that is to

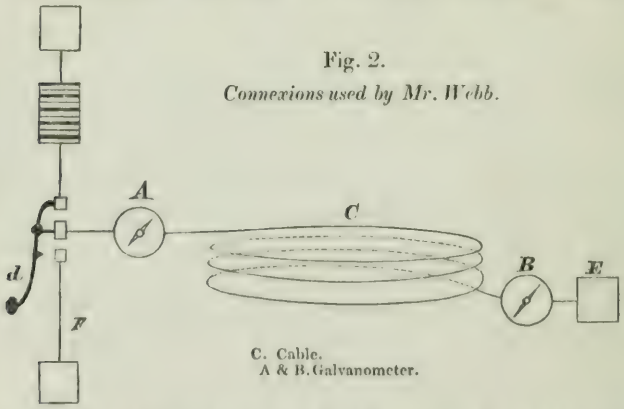

Fig. 2.

Connexions used by Mr. Webb.

say, putting what we have called the near end of the cable to earth] that the greatest and most singular difference occurs [between straight and coiled cables]. It will then be seen that the needle at A [that is to say, the needle of a galvanometer in circuit between key and cable instead of between key and earth, as in our experiments], instead of being reversed will continue

* Of the same nature as those observed by Prof. Wheatstone on his short cables.

deflected in the original direction, and both needles will very gradually resume the perpendicular."

"There is a most marked difference between the effect produced between a coiled and a straight cable. The return current appears obliterated, or rather it is overpowered by the effects of the inductive action which takes place from coil to coil. The deflection thus produced is much greater than that produced by the return current. I have had perhaps peculiar facilities for observing this striking phenomenon. Whilst picking up a cable at sea, I frequently test the length I am operating on for return current; *and as the cable becomes coiled into the ship the deflection of the needle, when testing for return current, becomes reversed.*

"It is also my practice to cut the cable at certain distances as it is picked up, and then test such sections separately. On these occasions, sections which, when one end is insulated, will give a charge and discharge of 5°, will when that end is to earth, give a current at the battery end, after contact, of 90°, but in the reverse direction to that in which the discharge or return current would be if the cable were laid out straight."

XXVI. *On the Movements of Gases.* *By* J. A. Wanklyn, *Demonstrator of Chemistry in the University of Edinburgh**.

WHEN a gas heavier than air is placed in a cylindrical vessel closed at the top and open at the bottom, it does not descend rapidly. In like manner, a gas lighter than air contained in a cylinder closed below, but freely communicating with the atmosphere above, does not move upwards with rapidity.

By simply placing a gas in a vertical cylinder shut at one end, the ordinary course of gravitation is disturbed—to how great an extent few people would anticipate.

The following experiments show how remarkably the fall of gases is retarded by such an arrangement.

A tube† filled with carbonic acid was allowed to remain with its mouth open and directed downwards for the space of five seconds. After the lapse of that time, the gaseous contents of the tube were analysed in order to ascertain how much carbonic acid had made its escape.

The composition of the gas was—

Air	26·3
Carbonic acid . . .	73·7
	100·0

* Communicated by the Author.

† Dimensions of the tube:—Diameter, 14·5 millims.; length, 232 millims.; capacity, 37 cubic centims.

A second experiment with the same tube, and the same length of exposure, gave—

Air	20·3
Carbonic acid . . .	79·7
	100·0

A third experiment, also with the same tube and same exposure, gave—

Air	26·6
Carbonic acid . . .	73·4
	100·0

Three more experiments, where the time of exposure was twenty seconds, other conditions remaining unaltered, gave—

	I.	II.	III.
Air	49·67	60·68	51·34
Carbonic acid	50·33	39·32	48·66
	100·00	100·00	100·00

The differences observed between the quantities of gas which escaped in the same times in the different experiments are no doubt caused by the action of currents of air which are produced by the act of unclosing the tube, which currents are necessarily variable in extent and direction. Though it would be idle to draw any inference as to the precise numerical relation subsisting between times of exposure and quantities of gas fallen, yet these oscillations in our different experiments do not at all affect the certainty of the general result.

In five seconds about one quarter of the carbonic acid escaped; in twenty seconds about one half.

Let us translate this into retardation of the fall of the gas.

Our tube was 232 millimetres long. Therefore, in five seconds three-fourths of the gas cannot have fallen more than 60 millimetres in vertical distance. By comparing this with the distance through which carbonic acid, contained in a balloon, would fall in five seconds, we arrive at an estimate of the retardation which we seek to measure.

At first sight, the cause of this retardation would appear to be friction between the carbonic acid and air which must enter to supply the place of the carbonic acid. But if we carefully consider the conditions under which we find the gases in these experiments, we shall see that another explanation is possible.

For, notwithstanding the absence of cohesion in gases, the carbonic acid in our experiment seems to be very much in the condition of water in the two arms of an equal-armed siphon; the essential difference between the two cases being that, whereas the cohesion between the particles of water hinders all move-

ment whatever, the want of cohesion in the gas permits movement, which accordingly takes place, but with exceeding slowness, and indeed (if certain theoretical conditions could be realized) with infinite slowness.

If we could place our gases in contact without occasioning any current by the act of making them communicate, and besides could realize—

(1) The inferior surface of the gas a mathematically horizontal superficies.

(2) A vessel mathematically cylindrical and vertical;

(3) Molecules of the gas infinitely little and absolutely non-adherent either to one another or to the glass;

then, these conditions being granted, an infinitely prolonged time would be required for any finite fraction of the gas to fall.

At the beginning of the experiment it would be the lowest stratum of molecules alone whose gravitation would tend to cause motion. All the molecules, situated above the *plane* of *contact* between our gas and the air, would be in equilibrium, as the descent of one of them would involve the ascent of another.

During the first instant the lowest plane of carbonic acid would change place with the uppermost plane of air. Thus a plane of air-molecules would be interposed between the mass of carbonic acid above and a plane of carbonic acid-molecules below.

During instant the second, the isolated plane of carbonic acid molecules would change places with the adjacent air immediately below it, while simultaneously the lowermost stratum in the mass of carbonic acid would change with the isolated stratum of air.

We should thus have an isolated stratum of carbonic acid-molecules of infinitely small thickness travelling downwards through the air; and if it could be shown that this isolated lowest stratum would require eternity to traverse a finite vertical distance, it will follow, *à fortiori*, that a finite fraction of the carbonic acid would require eternity to fall a finite distance.

That a body of infinitely small vertical diameter requires an infinite time to fall through a finite portion of a medium may be thus proved.

Assign any finite time, *e. g.* a second. In a second a body falling *in vacuo* acquires a velocity of 32 feet per second. Let our body be conceived to enter a medium being charged with a velocity of 32 feet per second (which is consequently the product of a greater force than the gravitation during a second). In moving through any finite portion of the medium, the body would encounter an infinite number of times its weight of the medium. It would therefore have to communicate its motion to its weight multiplied by infinity.

Hence in traversing a finite space its velocity would become 32 feet divided by infinity. It would therefore require an infinite time to traverse a finite space. *A fortiori*, in a second it could not traverse a finite space.

In like manner, any finite time being assigned, it can be shown that in that time no finite space can be traversed.

We are thus led to expect that carbonic acid should not escape from a tube more rapidly when its mouth is turned downwards than when it is turned upwards. Nor does the fact, that in the actual experiment the escape was more rapid in the former position than in the latter, disprove the proposition; for in the experiment there is a very great imperfection: viz., it is impossible to open a tube without creating a current. When a current is set up, the gas moves *en masse*, and then it is quite conceivable how gravitation can increase the movement; so that the descent of a quarter of the gas in our experiments is no proof that if the tube could be opened without disturbance there would be a higher rate of egress than there is when simple diffusion acts.

In order to show experimentally that it is the upward current of air which produces the retardation, the following experiment was devised and executed.

A tube 9 to 10 millimetres in diameter and 242 millimetres long was provided with ground-glass plates, closing both top and bottom. The top was fixed on with tallow, and the tube used as though it consisted of a single piece. It was filled with mercury and inverted in the mercurial trough, when it proved to be tight. Carbonic acid was then introduced in the usual way. The tube charged with that gas was then closed with the other ground-glass plate and removed from the trough. The top was taken off. The bottom was then removed and replaced in five seconds. The top was then put on, and the tube taken back to the trough, and its gas passed into a graduated tube and examined. It consisted of almost pure air: 91·111 vols. left 90·665 vols. not capable of absorption by potash.

This shows clearly that, however we may account for the mode of action of the upward current of air, it is the upward current which produces the remarkable retardation forming the subject of this paper.

Here it may be well to mention that a trial was made to ascertain the extent of movement produced by simple diffusion. The tube employed in the six first experiments was filled with carbonic acid and exposed, mouth opening upwards, for five seconds.

Only about 3 per cent. of carbonic acid had left the tube,

and probably even that small amount was chiefly due to the disturbance on opening the tube.

The following experiment was made with hydrogen (not dried). The same tube as was used in the six first experiments was filled with that gas and exposed for five seconds, with the open end upwards. The residual gas after the experiment contained—

Air	38·8
Hydrogen	61·2
	100·0

It will be obvious, on a little consideration, that the same causes are in operation in this instance as in the former instances where carbonic acid refused to descend.

To show that the same phenomena occur in very wide tubes, the following experiments may be cited.

A tube, 38 millimetres in diameter, 256·5 cubic centimetres in capacity, but of the same length (232 millim.) as the former tube, was filled with carbonic acid.

The residues in different experiments consisted of—

	Exposure of 5 seconds.		Exposure of 10·5 seconds.
	I.	II.	
Air	43·4	52·5	76·1
Carbonic acid	56·6	47·5	23·9
	100·0	100·0	100·0

The loss of carbonic acid is therefore greater than when the narrow tube is employed. We may explain this by the greater extent of current, which is of necessity produced on unclosing a wider tube.

Lastly, an experiment may be brought forward in which a mixture of gases was used, and in which the point aimed at was, whether or not relative change takes place in the composition of a mixed gas.

The mixed gas was prepared by heating oxalic acid with sulphuric acid*. An analysis previously to the experiment gave—

Carbonic acid	50·62
Carbonic oxide	49·38
	100·00

After exposure in the narrow tube opening downwards for 60 seconds, the product contained—

* No doubt a trace of SO^2 was present, but this it was not deemed necessary to remove.

Carbonic acid	17·91
Carbonic oxide	18·94
Air	63·15
	100·00

From which we see that little or no change in the relative proportions of $\mathrm{C\,O^2}$ and $\mathrm{C\,O}$ had taken place,—a circumstance tending to prove that the exit of gas, *en masse*, is due to currents and not molecular.

XXVII. *On some Cerium Compounds.* By M. Holzmann, *Ph.D.**

ON continuing my former researches on the cerium compounds†, I found a new class of double nitrates of cerium which do not contain the cerium in the state of protosesquioxide, but simply as protoxide. I prepared the cerium double salts of ammonium, potassium, strontium, magnesium, zinc, manganese, nickel, cobalt, and uranium, partly by dissolving the metal in a solution of nitrate of protosesquioxide of cerium containing a considerable quantity of free acid, partly by mixing the solutions of the two nitrates. The deoxidation of the protosesquioxide of cerium was effected in the first case by the hydrogen generated in dissolving the metal; in the second by boiling the nitrate of protosesquioxide of cerium with alcohol. If the solution of the cerium-salt contains an excess of nitric acid, the alcohol must be added in small quantities, as the disengagement of gas causes a violent ebullition. The analyses of the ammonium, magnesium, zinc, manganese, nickel, and cobalt salts were already finished, when a paper was published by L. Th. Lange‡, in which the same salts are described,—in consequence of which I have discontinued my research, and will now only state those of my observations which do not agree with Lange's.

The double salt of *nitrate of cerium* and *nitrate of magnesium*, prepared by myself, is not of a pale pink colour, but perfectly colourless, and only contains *six* atoms of water of crystallization. As this composition differs from that of the other salts belonging to the same group, I analysed the products of several preparations, but always obtained the same results. The salt was obtained by mixing equal parts of concentrated solutions of nitrate of magnesium and nitrate of protoxide of cerium, and leaving the mixture to crystallize over caustic lime and chloride of calcium. The crystals, representing perfectly developed hex-

* Communicated by the Author.

† *Journal für Praktische Chemie*, vol. lxxv. p. 321.

‡ Ibid. vol. lxxxii. p. 129.

agonal plates, frequently of one or two centimetres in diameter, were recrystallized three or four times, and the formation of large crystals prevented by stirring. For analysis, the salt was dried over caustic lime and chloride of calcium.

(1) 0·6942 grm., treated with recently precipitated oxide of silver, gave, after precipitating the filtered liquid with hydrochloric acid, 0·4156 Ag Cl. The liquid filtered off from the chloride of silver gave with ammonia and phosphate of sodium, 0·1690 $(MgO)^2 PO^5$. After treating the oxide of silver on the filter with hydrochloric acid, the liquid gave, on precipitation with oxalate of ammonium and ignition of the oxalate of cerium, 0·1647 $Ce^3 O^4$*.

(2) 0·52 grm., dissolved in water and precipitated by oxalate of ammonium, gave 0·1235 $Ce^3 O^4$: the filtrate gave with ammonia and phosphate of sodium 0·1237 $(MgO)^2 PO^5$.

(3) 0·4527 grm. gave, after Dumas's method (the substance in a platinum tray), 43·09 cubic centims. nitrogen of 0° C., and 760 millims. pressure. The residue, treated with concentrated nitric acid and precipitated by oxalate of ammonium, gave 0·1078 $Ce^3 O^4$; and the filtrate with ammonia and phosphate of sodium 0·1093 $(MgO)^2 PO^5$.

(4) 0·9543 grm., dissolved in water and precipitated with oxalate of ammonium, gave 0·2307 $Ce^3 O^4$, and the filtrate on evaporation and ignition 0·0778 MgO.

(5) 0·5343 grm., treated in the same manner, gave 0·1285 $Ce^3 O^4$ and 0·045 MgO.

These numbers lead to the formula

$$CeO, NO^5 + Mg O, NO^5 + 6 HO.$$

	Theory.		Experiment.				
			(1)	(2)	(3)	(4)	(5)
CeO . .	54	22·88	22·61	22·63	22·69	23·04	22·92
MgO . .	20	8·48	8·77	8·58	8·70	8·15	8·42
NO^5 . .	54	22·88	22·53	.....	46·13		
NO^5 . .	54	22·88	.....	.....			
6 HO . .	54	22·88					
	236	100·00					

The *double nitrate of cerium and ammonium* has not been prepared by Lange. It is obtained by mixing equal parts of rather concentrated solutions of the two salts, concentrating the liquid on the water-bath, and allowing it to cool over chloride of calcium and caustic lime. If the liquid cools gradually, it solidifies to a radiated crystalline mass; but if cooled quickly by

* It appears from this that only the nitrate of cerium gives up its acid to the oxide of silver; the quantity of chloride of silver found corresponds therefore only to half the quantity of nitric acid contained in the salt.

stirring, the crystalline powder may easily be separated from the mother-liquor. The salt is perfectly colourless, very soluble in water and alcohol, and exceedingly deliquescent in moist air.

(1) 0·5585 grm., three times recrystallized from water and dried over caustic lime and chloride of calcium, gave, after evaporation with hydrochloric acid and bichloride of platinum and ignition of the ammonio-chloride of platinum, 0·1485 Pt. After treating the filtrate with sulphuretted hydrogen and precipitating it with oxalate of ammonium, 0·1702 Ce^3O^4 were left on the ignition of the oxalate of cerium.

(2) 0·4325 grm., treated in the same way, gave 0·112 Pt.

(3) 0·3138 grm., precipitated with oxalate of ammonium, gave 0·0977 Ce^3O^4.

(4) 0·5905 grm. gave, after Dumas's method, 74·44 cubic centims. nitrogen of 0° C., and 760 millims. pressure.

These numbers are represented by the formula

$$2(CeO, NO^5) + NH^4O, NO^5 + 8HO.$$

	Theory.			Experiment.			
				(1)	(2)	(3)	(4)
2CeO . .	108	29·35		29·04	.....	29·67	
NH^4O . .	26	7·06	N^3 15·22	6·98	6·80	.....	15·84
$3NO^5$ . .	162	44·02		.....	.....	.....	
8HO . .	72	19·57					
	368	100·00					

In addition to the double salts of the *nitrate of proto-sesquioxide of cerium*, formerly described, I have prepared the *ammonium-salt*, which corresponds in composition and properties to the potassium-salt.

A mixture of the solutions of the two salts crystallizes, when left over caustic lime and chloride of calcium, in orange-red crystals, which have the appearance, under the microscope, of hexagonal prisms; this double salt is exceedingly deliquescent. The salt, recrystallized several times from water and dried over lime and chloride of calcium, gave on analysis the following results:—

(1) 0·4285 grm., treated as the ammonium double salt of the protoxide of cerium, gave 0·1477 Pt, and 0·1265 Ce^3O^4.

(2) 0·5442 grm., treated in the same manner, gave 0·1832 Pt, and 0·16 Ce^3O^4.

(3) 0·4645 grm., ignited alone, gave 0·1385 Ce^3O^4.

(4) 0·5684 grm. gave, after Dumas's method, 91·52 cubic centims. N of 0° C., and 760 millims. pressure.

(5) 0·5633 grm., dissolved in water and precipitated with oxalate of ammonium, gave 0·1658 Ce^3O^4.

I thought it superfluous to determine the degree of oxidation of the cerium, as the values obtained agree exactly with the

formula $\left.\begin{matrix} Ce\,O \\ 2\,NH^4\,O \end{matrix}\right\} 3\,NO^5 + Ce^2\,O^3, 3\,NO^5 + 3\,HO.$

	Theory.			Experiment.				
				(1)	(2)	(3)	(4)	(5)
$Ce^3\,O^4$	170	29·67		29·52	29·40	29·32	.....	29·43
$2\,NH^4\,O$	52	9·08	N^8 19·55	9·05	8·91	.....	20·23	
$6\,NO^5$	324	56·54		.....	.....	.....		
$3\,HO$	27	4·71						
	573	100·00						

On trying to prepare a double salt of the nitrate of the proto-sesquioxide of cerium with nitrate of aniline, the latter was instantaneously oxidized, and at the same time a dirty green precipitate was formed. In this way even a very small quantity of the proto-sesquioxide may be detected; for the liquid, when dilute, directly assumes a red colour.

The nitrate of the protoxide of cerium seems to form double salts with the nitrates of some of the organic bases, with the investigation of which I am now engaged.

A mixture of the solutions of *protochloride of cerium* and *bichloride of platinum*, when highly concentrated, deposits on cooling orange-coloured crystals, easily soluble in water and alcohol, but insoluble in ether. They fuse in the water-bath, and are deliquescent in moist air. An alcoholic solution, when slowly evaporated over chloride of calcium, often furnishes perfectly developed rectangular prisms. For analysis, the salt was twice recrystallized from water and dried over caustic lime and chloride of calcium.

(1) 0·9615 grm., treated with sulphuretted hydrogen, gave, after filtering and igniting the sulphide of platinum, 0·2375 Pt. The filtrate, boiled and precipitated with oxalate of ammonium, gave 0·2671 $Ce^3\,O^4$. The remaining liquid, when treated with nitrate of silver, gave 1·3485 Ag Cl.

(2) 0·782 grm., dissolved in alcohol and mixed with chloride of ammonium, gave after ignition of the ammonio-chloride of platinum 0·1904 Pt. The filtrate, precipitated by oxalate of ammonium, gave 0·22 $Ce^3\,O^4$.

(3) 0·6215 grm., treated in the same manner, gave 0·151 Pt, and 0·1724 $Ce^3\,O^4$.

These numbers lead to the formula

$$(Ce\,Cl)^2\,Pt\,Cl^2 + 8\,HO.$$

	Theory.		Experiment.		
			(1)	(2)	(3)
2 Ce	92	22·72	22·55	22·84	22·53
Pt	99	24·44	24·70	24·35	24·30
4 Cl	142	35·06	34·69		
8 HO	72	17·78			
	405	100·00			

The double chlorides of manganese or magnesium with platinum differ from this composition; for their formula, according to Bonsdorff*, is M Cl, Pt Cl^2 + 6 HO.

When mixed solutions of *protochloride of cerium* and *iodide of zinc* are left for some time over chloride of calcium, a syrupy mass is generally obtained; very rarely a crystalline double salt is deposited from the solution. I have not succeeded in purifying this compound, as it attracts water with great avidity, and can hardly be recrystallized in consequence of its extreme solubility in water and alcohol. On concentrating a solution of the salt in the water-bath, iodine is liberated.

In conclusion I may mention that *oxalate of cerium, lanthanium*, or *didymium* may be obtained in perfectly developed rhombohedrons, attaining often a diameter of 2 or 3 millims., when dissolved in moderately concentrated nitric acid and allowed to evaporate slowly over caustic lime. An acid salt, however, is not obtained in this way, even when free oxalic acid is dissolved together with the oxalate: for 0·7968 grm. of oxalate of cerium, dried over caustic lime and chloride of calcium, left, on ignition, 0·3605 $Ce^3 O^4$, corresponding to 36·70 per cent. of cerium; and 1·3862 grm., burnt with oxide of copper, gave 0·4784 CO^2 and 0·4175 HO, corresponding to 9·41 per cent. of carbon, and 3·35 per cent. of hydrogen. The formula $C^4 O^8 Ce^2$ + 8 HO requires 36·51 of cerium, 9·52 of carbon, and 3·18 per cent. of hydrogen. When the nitric acid is employed in a too concentrated state, and when the solution is heated to ebullition, a partial decomposition takes place, and a mixture of crystals of the oxalate and of free oxalic acid is obtained.

New Lodge, August 1, 1861.

XXVIII. *Some Observations on the Sensibility of the Eye to Colour.* *By* John Z. Laurence, *F.R.C.S., M.B. Lond., Surgeon to the South London Ophthalmic Hospital*†.

IF, closing one eye—say the right—any highly luminous white ground, such as some portions of the sky on a sunny day, is viewed with the left through a dark tube so as to exclude all extraneous light, after a little the eye will begin to feel fatigued, and a librating circular smoky spectrum will be perceived at the end of the tube. When the tube is laid aside and

* Gmelin's *Handbuch*, vol. iii. p. 765 and 767.

† From the Glasgow Medical Journal, July 1, 1861. Communicated by the Author.

[Since writing this paper, my attention has been directed to a series of elaborate disquisitions by Brücke and Fechner in Poggendorff's *Annalen der Physik und Chemie*, vols. xliv., l., and lxxxiv., to which I beg to refer my readers.—J. Z. L.]

both eyes are directed to the sky, a similar spectrum will be observed, projected, as it were, on the surface of the heavens, but much darker. But if after a time each eye is alternately opened and closed, a rose-coloured spectrum is seen with the left eye, a pale green one with the right. These appearances are seen still better if, instead of the sky, a white screen is used as the plane of projection in the second part of the experiment. At first an almost black circular disc is seen; this becomes lighter and lighter, till it is finally succeeded in the left eye by a bright rose-colour disc, surrounded by a violet border; in the right eye by an equally bright green with a rose border. These spectra sometimes appear as if upon the surface of the screen, sometimes, on the contrary, as if originating within the eyeball itself, and indeed may be even seen with both eyes closed. To see the above phenomena in all their intensity, a slightly different plan must be adopted. As the field of projection, a sheet of dead black paper in a dark room is to be used; the spectra then seen with either eye are the same, and their colours most splendid, both as regards brightness and tint. At first an emerald-green disc appears, surrounded by a narrow carmine, or perhaps, more accurately, magenta border; the magenta tint is then seen to encroach more and more upon the green, till the whole disc is of the former colour, surrounded by a bluish-violet border; this last, in its turn, invades the magenta, till the final spectrum is of one uniform indigo-violet colour.

The above is the general sequence of colours which I, and other persons whom I have asked to perform the experiment, have observed; but these are liable to exceptions. Occasionally, the librating spectrum observed at the end of the tube in the first part of the experiment, acquires a faint rose, green, or violet tint. Sometimes I have seen the spectra of the right and left eyes, in the second part of the experiment, reversed as regards colour.

These facts appear to prove the following propositions:—

1. That colour sensations may be excited in the retina, or brain, altogether independently of any external colour-stimulus.

2. That as an *optical* analysis of white light may be effected by a prism, so with the eye we possess the power of effecting, what may be called, its *physiological* analysis.

3. The last proposition tends to the conclusion that white light consists of three fundamental colours—magenta, emerald-green, and indigo-violet—corroborating in a remarkable manner the opinions of Professor Maxwell and Dr. Young on the same subject.

4. That a colour sensation excited in one eye is generally felt in the other, although this latter has not been exposed to

influence of light in any part of the experiment; that, in a word, a very close sympathy exists in the two retinæ, of which the consentaneous action of the two irides is probably but a reflex nervous consequence.

I may here allude to a distinction in ocular spectra which has, I believe, not been taken much account of by observers of these phenomena. Some spectra seem as if projected on the plane to which we direct the eye, and in that case appear, as I have found from numerous measurements, linearly magnified in proportion to the distance of the eye from the plane of projection. Other spectra, on the contrary, are perceived, so to say, in the eyeball itself, and are of a subjective nature. Independent of the differences of their apparent seats, the two classes of spectra present certain other well-defined distinctions. Projected spectra are only perceived with the eyes open, and are generally but faint in colour; while subjective ones may be seen with the eyes shut, and are always intense in colour. At the same time I am disposed to ascribe the differences of colour, in a certain degree, to the diluting influence of extraneous light; for projected spectra are always seen more vivid in a dark room than in daylight.

The green spectrum observed on a sheet of white paper, after prolonged contemplation of a red wafer, has been commonly explained thus:—"When the eye has been for some time fixed on the *red* wafer, the part of the retina occupied by the red image is deadened by its continued action, and insensible to the red rays which form part of the white light from the paper; consequently will see the paper of that colour which arises from all the rays in the white light of the paper, but the red; that is, of a *bluish-green* colour, which is therefore the true complementary colour of the *red* wafer*."

That this explanation is not correct seems to me to be proved by the following experiment:—

I, at night, made a room (which is provided with thick American-leather blinds for ophthalmoscopic purposes), to all appearance, absolutely dark, then viewed with the left eye a small aperture in a dark box covered with a piece of emerald-green glass, behind which was the nearly white flame of a lamp. The right eye was kept closed, and covered with a thick handkerchief. After a time I blew out the light in the box, and looked at a screen covered with a sheet of dead black paper. With the left eye a large carmine-coloured projected spectrum of the flame could be seen; with the right eye I generally perceived no spectrum at all, or if any, but of a very faint tint. But if the latter eye was exposed to a white light during the first part of the ex-

* Brewster's 'Optics,' 1831, p. 305.

periment, I invariably perceived the same spectrum with this eye as I did with the left one.

This experiment shows that the presence of white light is not necessary for the perception of complementary ocular spectra, and further, would appear to indicate that for a sympathetic spectrum to be excited in the eye which has not been exposed to the colour-stimulus, the excitation of some light is necessary.

M. Plateau painted one half of a piece of paper red, the other green; and after alternately directing the eyes to each half, covered them with a handkerchief, and observed a black image, having on each side a complementary-coloured image*. He hence inferred that "the combination of accidental colours produces black." Sir D. Brewster very properly objects to this conclusion, "because the eye has been in succession rendered insensible to the two colours which compose white light itself†." Elsewhere the same author says, "If we take the two complementary colours, namely, the *red* and the *green* tints forming the *ordinary* and *extraordinary* pencils in the polarized ring, which, by overlapping, form *white light*, then it is manifest that the accidental colour of the overlapping part is *black*, and hence the sum of the action of the *red* and *green* acting separately must also be black‡."

Notwithstanding, however, the authority of Sir D. Brewster, the following experiment which I have performed appears to me rather to corroborate Plateau's view. If the two halves of a card painted red and green respectively be illuminated by a green or red light, they appear black. In the same way, but depending on a different cause, the two halves of the card, if viewed through green or red glass, appear black.

Another set of observations, connected in a degree with the preceding, may be here noticed. Chevreul§ distinguishes two chief species of contrast of colours, *simultaneous* and *successive* contrasts. But an examination of these distinctions shows them in my judgment to be more apparent than real, and but the expression of one fundamental fact, viz. that the eye on perceiving any one colour acquires a tendency to see its complementary. Thus, to take an example of Chevreul's simultaneous contrast:—If a slip of red and one of yellow paper be viewed side by side, near the line of contact the red paper inclines to violet, the yellow to green. The *rationale* of this is at once obvious: the red mingling with the complementary of yellow,

* *Annales de Chimie* for 1833.

† Lond. and Edinb. Phil. Mag. for May 1839, p. 335.

‡ *Op. cit.* for December 1839, p. 437.

§ The Principles of Harmony and Contrasts of Colour, by M. E. Chevreul.

i. e. blue, produces the violet tint; whilst the yellow mingling with the complementary of red, *i. e.* green, produces a light green; and this same law holds good in the juxtaposition of any two colours whatever. By the term successive contrast Chevreul designates the familiar phenomena of complementary ocular spectra, of which a most comprehensive history has been given by Darwin in the Philosophical Transactions, vol. lxxvi. p. 33 *et seq.* Du Tour* thought that the two eyes cannot perceive each a separate colour at once. He says that if, *e. g.*, a blue disc be presented to one eye and a yellow one to the other, the result is that the mind perceives alternately the one or the other colour, but not the two at once. But I would submit that these two statements do not include the whole facts of the case. I took two tubes, each $10\frac{1}{2}$ inches long, and applying the end of one to each eye, viewed the sky through them. I found that when the contiguous edges of the tubes at their further ends were some inches apart, two distinct white circles of sky were seen; these circles touched when the edges of the tubes were from $2\frac{1}{4}$ to $2\frac{1}{8}$ inches apart, and, when closer, the two circles appeared as one. If now the further end of one tube was covered with a piece of green glass, the end of the other with a piece of red, as long as the ends of the tubes were kept not closer than $2\frac{1}{4}$ to $2\frac{1}{8}$ inches asunder the two coloured discs were perceived perfectly distinct from one another; no alternation of either colour to the exclusion of the other, as in Du Tour's experiment, ensued, so long as the tubes were inclined to each other at this or any greater degree of divergence.

Another very interesting series of phenomena depending on the intrinsic sensibility of the eye to the impressions of colours, are those of coloured shadows. The first exact observations on these were made by Count Rumford†. He observed that the two shadows of an object placed in front of a white ground, from a white and a coloured light, were of the two colours complementary to the latter. I have investigated this fact a little more closely. The method adopted has been to throw a white and a coloured (red) circle of light from two magic lanterns on a white screen, before which a slender wooden rod was placed. It is easy to satisfy ourselves that the red shadow is produced by the (otherwise colourless) shadow cast from the interception of the white light being simply illuminated by the other red light. The green shadow is the shadow produced by the interception of the red light, illuminated by the white light. These coloured sha-

* *Mémoires de Mathématique et de Physique présentés à l'Académie Royale des Sciences*, vol. iii. p. 514; iv. p. 499. Paris, 1760-63.

† Philosophical Papers by Benjamin, Count of Rumford. London, 1802, vol. i. p. 333.

dows have, by Rumford and many subsequent observers, been ascribed to the effect of contrast. But this appears an inadequate explanation; for if, with one magic lantern, a half-white and a half-red circle of light be thrown on a screen, a shadow thrown across the two fields is simply dark, without any colour at all. If, again, a red and a white disc of light be thrown from two magic lanterns respectively on a screen, so as partially to overlap, where the overlapping takes place two complementary shadows of any object are seen, but in the other two parts of the field only one colourless dark shadow is seen.

The following facts seem to form the basis of the explanation of coloured shadows:—First, the experiment of Rumford*,—that a piece of grey paper placed next to a piece of coloured paper, both on a black ground with the exclusion of extraneous light, appears tinged with the complementary colour. Secondly. I found by my own experiments that if, in a dark room, the screen is illuminated with a red circle of light from a magic lantern, the greenness of the shadow and the redness of the ground on which it appears are inversely proportional to one another. By approximating the red light to the screen this becomes redder, whilst the shadow of the rod placed before it becomes less green and darker, till it becomes an ordinary black shadow; that, on the other hand, removing the red light till it leaves the white screen but faintly tinged with red, brings out the green shadow very prominently, and on admission of light into the room, a second faint red shadow comes out.

Meusnier observed "that when the sun shone through a hole a quarter of an inch in diameter on a red curtain, the image of the luminous spot was green." Another observer, Mr. Smith of Fochabers†, states, "If we hold a narrow strip of white paper vertically, about a foot from the eye, and fix both eyes upon an object at some distance beyond it, so as to see it double, then if we allow the light of the sun, or a light from a candle, to act strongly upon the right eye without affecting the left, which may be easily protected from its influence, the left-hand strip of paper will be seen of a bright *green* colour, and the right-hand strip of a *red* colour."

From all these facts, I think the conclusion arrived at by Sir David Brewster appears highly probable, that "as in acoustics, where every fundamental sound is actually accompanied with its harmonic sound, so in the impressions of light, the sensation of one colour is accompanied by a weaker sensation of its accidental

* *Op. cit.* p. 336.

† Brewster's 'Optics,' p. 405. Lond. and Edinb. Phil. Mag. for October 1832, vol. i. p. 249.

or harmonic colour*." To this might perhaps be added, that there is a tendency in the eye to, as it were, decompose white light into two complementary colours; and further, that the predominant decomposition is into red and green.

Applying this theory to the phenomena of coloured (*e. g.* red and green) shadows, the red shadow has already been shown to be simply due to the illumination of a colourless shadow by a red light; whilst on the whole of the rest of the field of the white screen, the red tint cast from the magic lantern is sufficiently powerful to overcome the green tint which the eye would otherwise perceive, excepting at one spot, namely, that which does not receive any red light on account of the interposition of the opake rod. Here the green (harmonic) colour, having no antagonistic red to overcome it, is rendered sensible to the eye.

XXIX. *On the Measurement of Electric Resistance according to an absolute Standard.* By Wilhelm Weber†.

§ 1. *Explanation of the absolute unit of measure for Electric Resistances.*

If there are measures for time and space, a special *fundamental measure* for *velocity* is not necessary; and in like manner no special *fundamental measure for electric resistance* is needed if there are measures for electromotive force and for intensity of the current; for then *that resistance can be taken as unit of measure,* which a closed *conductor possesses in which the unit of measure of electromotive force produces the unit of measure of intensity.* Upon this depends the reduction of the measurements of electric resistance to an absolute standard.

It might be thought that this reduction would be more simply effected by reverting to the special dimensions, length and section, and adhering to that metal (*copper*) which is best fitted and is most frequently used for such conductors. In that case the absolute unit of measure of resistance would be that resistance which a copper conductor possesses whose length is equal to the measure of length, and whose section is equal to the measure of surface, in which, therefore, besides measure of length and surface, *the specific resistance of copper must be given as unit* for the specific resistance of conducting substances. Thus a special

* Brewster's 'Optics,' p. 309.

† Translated from Poggendorff's *Annalen*, vol. lxxxii. p. 337, by Dr. E. Atkinson. [From the great scientific and practical importance which the determination of electric resistances has of late acquired, it has been thought advisable to give a translation of Weber's original paper published in 1851, containing the method of referring these resistances to an absolute standard.—Eds.

fundamental measure for specific resistances would be necessary, the introduction of which would be open to question. *First*, because there would be no saving in the number of the fundamental measures if, in order to do without a fundamental measure for the absolute resistance, another fundamental measure must be introduced which is otherwise superfluous. And *secondly*, neither copper nor any other metal is fitted for use in establishing a fundamental measure for resistances. Jacobi says that there are differences in the resistances of even the chemically purest metals, which cannot be explained by a difference in the dimensions; and that, accordingly, if one physicist referred his rheostat and multiplicator to copper wire a metre in length and 1 millimetre thick, other physicists could not be sure that his copper wire and theirs had the same *coefficient of resistance*, that is, whether the *specific* resistance of all these wires was the same. The reduction of measurements of galvanic resistances to an absolute measure can therefore only have an essential importance, and find a practical application, if it takes place in the first mentioned way, in which no other measures are presupposed than those for *electromotive force* and for *intensity*.

The question then arises, as to what are the measurements of *electromotive forces* and *intensities*? In measuring these magnitudes, no specific *fundamental measures* are requisite, but they can be referred to *absolute measure* if the magnetic measures for *bar magnetism* and *terrestrial magnetism*, as well as *measure of space and time*, are given.

As an absolute unit of measure of electromotive force, may be understood *that electromotive force which the unit of measure of the earth's magnetism exerts upon a closed conductor, if the latter is so turned that the area of its projection on a plane normal to the direction of the earth's magnetism increases or decreases during the unit of time by the unit of surface*. As an absolute unit of intensity, can be understood *the intensity of that current which, when it circulates through a plane of the magnitude of the unit of measure, exercises, according to electro-magnetic laws, the same action at a distance as a bar-magnet which contains the unit of measure of bar magnetism*. The absolute measures of *bar magnetism* and of *terrestrial magnetism* are known from the treatise of Gauss, "Intensitas Vis Magneticæ Terrestris ad mensuram absolutam revocata," Göttingæ, 1833 (Poggendorff's *Annalen*, vol. xxviii. pp. 241 and 591).

From this statement it is clear that the measures of electric resistances can be referred to an absolute standard, provided measures of *space*, *time*, and *mass* are given as fundamental measures; for the absolute measures of *bar magnetism* and of *terrestrial magnetism* depend simply on these three fundamental measures. A

closer consideration shows that even of these three fundamental measures, the *measure of mass* does not come into consideration, as follows from the following summary of the simple relations which are established by the determination of the absolute measures of these various kinds of magnitude.

As fundamental measures, there are to be considered the *measure of length* R, and the *measure of time* S; as *absolute measures*, the superficial measure F, and the units of measure of *bar magnetism* M, of *terrestrial magnetism* T, of *electromotive force* E, of *intensity* I, and of *resistance* W.

Hence, *first*, if wW is the resistance of any closed circuit, eE the electromotive force acting upon this conductor, and iI the intensity of the current produced by this electromotive force, we have the relation between the three numbers

$$w = \frac{e}{i};$$

from which it is clear that if the numbers e and i are determined, the number w is also indirectly obtained without needing a special determination.

Secondly, let eE stand for the electromotive force which acts upon any closed (plane) conductor, fF the area of the plane enclosed by this conductor, tT the earth's magnetism on which the electromotive force depends; and let sS express the space of time in which the plane of that conductor is moved by rotation from a position parallel to the direction of the earth's magnetism to a position at right angles to it, in such a manner that the limited surface produced by its projection on a plane at right angles to this direction of the earth's magnetism increases by the unit of measure during the unit of time proportional to the time. We shall then have between these four numbers e, f, t, s, the following relations,

$$e = \frac{ft}{s};$$

and hence it is clear that if the three numbers f, t, s are determined, the number e is also thereby directly given without necessitating a special measurement.

If, *thirdly*, iI is the intensity of the current in any closed conductor, fF the area of the plane enclosed by this conductor, and mM the magnetism of a bar which, when substituted for that conductor (its magnetic axis at right angles to the plane of the conductor), exercises the same actions at a distance, according to electro-magnetic laws, as that conductor, the following relation obtains between the three numbers i, f, and m,

$$i = \frac{m}{f};$$

from which it follows that if the numbers f and m are determined by measurement, i can be directly obtained without a special measurement.

From these three relations we get, finally,

$$w = \frac{e}{i} = \frac{fft}{sm};$$

hence if the four numbers f, s, m, t are determined, the number w is also directly obtained. The number f is obtained by measuring the area of the plane embraced by the conductor; s is found by measuring the time; and there only remain the numbers m and t, which are obtained by measuring the bar magnetism by the method described by Gauss in the above paper. The unchangeability of the unit of measure for electric resistance can accordingly be guaranteed so long as the four given measures (space, time, and the units of measure for the earth's magnetism and for bar magnetism) are obtained unchanged. But it by no means follows that the maintenance of these four given measures is a necessary condition for the unchangeability of the unit of measure of electric resistances; the simple maintenance of that unit of measure for *velocities* is sufficient for the purpose.

For if tT is the earth's magnetism, on which the electromotive force depends, which acts upon the closed conductor whose resistance has been measured; if, further, m'M is the magnetism of a bar (whose magnetic axis is parallel to the direction of the earth's magnetism, while the straight line drawn from its centre to the centre of the plane enclosed by the conductor is normal thereto) which, according to magnetic laws, would, from a great distance, exert the same action as tT the earth's magnetism; and, finally, if Rr is the length of the straight line drawn from the middle of this bar to the middle of the plane enclosed by the conductor, we have, according to the *Intensitas*, the simple relation

$$t = \frac{m'}{r^3}.$$

Substituting this value of t in the equation for w, we have

$$w = \frac{ft}{r^3} \cdot \frac{m'}{m} \cdot \frac{1}{s}.$$

If, finally, r'R is the side of a square whose area is equal to the area of the plane enclosed by the conductor, from which is obtained the relation

$$f = r'r',$$

and substituting this value of f in the above equation, we have

$$w = \frac{r'^3}{r^3} \cdot \frac{m'}{m} \cdot \frac{r'}{s}.$$

It is self-evident that a change of the given measures has no influence on the value of the factor $\left(\frac{r'^3}{r^3} \cdot \frac{m'}{m}\right)$; but a change of the given measures of time and space does influence the value of the factor $\frac{f'}{s}$, and accordingly the value of the number w, if both measures are not simultaneously increased or diminished in proportion. The value of the number w is hence quite independent of all alterations of the given measures, so long as there is no change in the *measure of velocity*. But if, by an alteration of the given measures, the standard of velocity is increased or diminished n times, an n times larger or smaller value is obtained for the factor $\frac{r'}{s}$, and therefore also for the number w, which is as much as to say that the resistance in this case is expressed according to an n times smaller or larger standard. The unchangeability of the unit of measure for resistance merely depends therefore on the unchangeability of the given measure of velocity. But if the measure of velocity is taken n times larger or smaller, the unit of measure for resistance becomes simultaneously n times larger or smaller.

§ 2. *Method of measuring Electric Resistance according to an absolute standard.*

The measurements of length and of time, which, according to the preceding paragraph, are adequate for the determination of electric resistance, presuppose circumstances on the convenient arrangement of which the practical execution and accuracy of such a determination depend. The following arrangement may serve as a simple summary of the essential circumstances.

Out of the galvanic conductor whose resistance is to be determined, two circular rings, A and B, are formed, which are connected in the manner represented in the figure. The whole conductor, consisting of the two circles A, B, and the junctions form a continuous line, of which it may be assumed,

for the sake of simplicity, that it is situate in one plane, and that the straight line connecting the centres of both circles coincides with the direction of the earth's magnetism. Let T be the force of the earth's magnetism as determined according to an absolute standard by magnetometric measurements; let r be the diameter of the circles, which, for simplicity sake, are assumed to be equal. If now the circle A is projected in the direction of the earth's magnetism AB on a plane normal to AB, the area of the projected plane is 0. From the flexibility of the wires connecting the

two circles, let it be supposed that the circle A is so twisted as to be at right angles to AB, in which case the area of the plane of the projection is πrr. Let this rotation take place in a short time s, in such a manner that the area of the plane of the projection of the circle increases uniformly in this time from 0 to πrr. From the *magneto-electrical laws*, an *electromotive force* results which the terrestrial magnetism T exerts upon the rotated circular conductor A during the time s, and which, according to the unit of measure explained in the preceding paragraph, is expressed by Ee, in which the number e is determined by the equation

$$e = \frac{\pi rr}{s} \cdot \mathrm{T}.$$

By this electromotive force a current is produced in the time s passing through the whole closed conductor, whose *intensity*, according to the unit explained in the preceding paragraph, is expressed by iI. This current passes also through the circle B, and acts from here on a distant magnetic needle in C, whose axis of rotation lies in the plane of the circle at right angles to the direction of the earth's magnetism. Let C lie in the produced AB (that is, the line joining the centres of the circles A and B). It follows now from *electro-magnetic* laws, that the momentum of rotation exerted on the needle at C by a current passing through the circle B, is equal to the rotation exerted by a bar-magnet placed in the centre of the circle in such a manner that its magnetic axis is at right angles to the plane of the circle, if its magnetism M, expressed according to absolute measure, is

$$\mathrm{M} = \pi rri.$$

If, further, the magnetism of the needle in C expressed in the same measure $= m$, and Bc = R, and ϕ the angle which the magnetic axis of the needle in C makes with the direction of the earth's magnetism AB, the momentum of rotation exerted by the bar magnetism M on the bar magnetism m is expressed, according to known magnetic laws, by

$$\frac{\mathrm{M}m}{\mathrm{R}^3} \cdot \cos\phi = \frac{\pi rr}{\mathrm{R}^3} \cdot im\cos\phi.$$

From which it follows that if K is the inertia of the needle, the *acceleration* of the rotation is

$$\frac{dd\phi}{ds^2} = \frac{\pi rr}{\mathrm{R}^3} \cdot \frac{im}{\mathrm{K}} \cdot \cos\phi;$$

and therefore that if the needle were previously at rest, and $\phi = 0$, the velocity of rotation at the end of the short time s is

$$\frac{d\phi}{ds} = \frac{\pi rr}{\mathrm{R}^3} \cdot \frac{im}{\mathrm{K}} \cdot s.$$

The greatest deflection α of the needle set in oscillation is known by *direct observation*; and the following expression is obtained for it from the above velocity, from known laws of oscillation, by multiplying by the length of oscillation t and dividing by the number π:

$$\alpha = \frac{rr}{R^3} \cdot \frac{im}{K} \cdot st.$$

For the length of oscillation we have the known equation

$$mT = \frac{\pi\pi K}{tt};$$

from which

$$\frac{mt}{K} = \frac{\pi\pi}{tT};$$

and thus

$$\alpha = \frac{\pi\pi rr}{R^3} \cdot \frac{is}{tT}.$$

Now α is obtained by direct observation; and hence for determining i we have

$$i = \frac{R^3}{\pi\pi rr} \cdot \frac{t}{s} \cdot T\alpha.$$

Remembering that the current passing through the circle B also traverses the circle A, we might also calculate the action of the circular current A upon the needle in C; but, for the sake of simplicity, it may be assumed that the distance AC is so great that this action vanishes in comparison with the action of the circular current B; in that case the *actually observed* deflection of the needle in C gives directly the value of α.

Consequently, by the *electromotive force* eE, expressed in an absolute measure, for which has been found the expression

$$e = \frac{\pi rr}{s} \cdot T,$$

a current is produced, in the whole closed conductor whose space is to be measured, the *intensity* of which is expressed in an absolute measure by iI, in which

$$i = \frac{R^3}{\pi\pi rr} \cdot \frac{t}{s} \cdot T\alpha$$

has been found. But, according to the unit explained in the preceding paragraph, the *desired resistance* of the whole closed conductor is expressed by wW, in which w is determined by the relation of the numbers e and i; for

$$w = \frac{e}{i} = \frac{\pi^3 r^4}{R^3 t \alpha}.$$

Hence the execution of the measurement of an electric resistance

depends on the measurement of the magnitudes

$$r,\ \mathrm{R},\ t,\ \alpha;$$

in other words, the resistance of the whole closed conductor can be expressed in an absolute measure, if by observations, *first*, the number α has been found which the deflection of the needle gives in parts of the diameter; *secondly*, the number $\frac{r}{\mathrm{R}}$, which gives the diameter of both circles in parts of the distance BC; *thirdly*, the velocity $\frac{r}{t}$, with which the diameter of those circles is traversed during one rotation of the needle. Hence it appears that the *measure of velocity* is the only measure which must be given if the resistance of a conductor is to be determined according to an absolute standard.

§ 3. *Observations.*

Of the four magnitudes which, according to the preceding paragraph, are to be found by *observation* for the purpose of determining electric resistances according to an absolute standard, three can readily be measured, namely, the diameter r of the two circles, the distance $\mathrm{BC}=\mathrm{R}$ of the circle B from the needle at C, and the time of oscillation of the needle t. There only remains the fourth magnitude, that is the deflection of the needle α expressed in parts of the diameter, and this is usually so small that it cannot be observed. This is the reason why, in actually making the observations, a slight deviation must be made from the arrangement described in the previous paragraph. For in order to obtain a value of α large enough for accurate observation, it is *first* necessary that the magnetic needle, upon which the circular current B is to act, instead of being at a great distance $\mathrm{BC}=\mathrm{R}$, be suspended in the centre of the circular current itself, in which case the action is the greater the smaller is the diameter r in comparison with R. Care must also be taken that the length of the needle is much smaller than the diameter of the circle, in order that the peculiar distribution of the magnetism in the needle need not be taken into account, because the investigation of this distribution is attended with difficulties. It is further necessary that both circles, instead of one, shall consist of several windings of the conductor, by which they become changed into rings of large diameter. In that case, however, the influence of all the windings must be individually taken into account, because they have different diameters, and are not all on the same plane as the needle.

For the conductor whose resistance was to be measured, a very long thick copper wire was chosen which weighed 169 kilo-

grammes. Of this 16 kilogrammes were used for the ring A, which consisted of 145 windings; enclosing altogether a surface of nearly 105 square metres. This ring was placed vertically, and by means of a winch could be rapidly rotated in a semicircle, so that the perpendicular upon the plane of the ring at the commencement and at the end of the rotation coincided with the magnetic meridian. The other 153 kilogrammes were used for the ring B, which consisted of 1854 windings, giving together a section 202 millims. in breadth, and 70·9 millims. in height: the internal diameter of this ring was 303·51, and the external 374·41 millims. This second ring was firmly fixed, and its plane coincided with that of the magnetic meridian. In the centre of this second ring B, a small magnetic needle 60 millims. long, provided with a mirror, was suspended by a filament of silk, as in a small magnetometer; and the oscillations and deflections of the needle were observed with a telescope, directed to the mirror, on a scale about 4 metres from the mirror.

The observations were made in the following manner. The ring A was first so placed that its plane coincided with the magnetic meridian, and the needle in the middle of the ring was thereby brought to rest; thereupon the ring A was suddenly turned 90°. By this means the needle in the middle of the ring was set in rotation, and by means of the telescope the position of the needle was observed on the scale at its greatest (positive) deflection after half an oscillation. After a complete oscillation, and therefore an oscillation and a half after the beginning, the needle attained its greatest deflection on the opposite side, which was also observed on the scale. In the moment at which the needle passed its original position of rest, and therefore two oscillations after the beginning of the experiments, the ring A was rotated 180°. The oscillating needle was thereby arrested in the middle of its motion, and thrown backwards, upon which its greatest negative and greatest positive deflections were observed on the scale. After the expiration of four oscillations from the commencement, that is, at the moment at which the needle returning from its last deflection passed its original position of rest, the ring was again turned forwards by 180°, and then the same oscillation observed as in the first case, and in this manner the experiments were continued until a sufficient series of observations was obtained. For each series, in the *first* column of the following Table are given the deflections observed on the scale and arranged in order under one another; in the *second* column the mean between two successive positive or negative deflections are added. In the *third* column are the differences of the means referring to positive and negative deflection, that is, the magnitude of the whole arc.

First Series.			Second Series.			Third Series.		Fourth Series.		
467·1			467·1			463·0		462		
540·7	543·70		540·5	543·65		536·7	539·65	534·7	538·20	
546·7		80·10	546·8		79·65	542·6		541·7		80·00
461·4	463·6		461·3	464·00		456·6	459·25	455·3	458·20	
465·8		79·75	466·7		79·55	401·9		401·1		79·75
540·6	543·35		540·8	543·55		537·6	539·60	535·1	537·95	
546·1		79·25	546·3		79·90	541·6		540·8		79·50
462·3	464·10		461·8	463·65		458·3	460·05	456·0	458·45	
465·9		79·45	465·5		80·00	461·8		400·9		79·50
541·4	543·55		542·1	543·65		537·7	539·75	535·3	537·95	
545·7		79·75	545·2		79·70	541·8		540·6		80·05
462·3	463·80		462·8	463·95		457·9	459·80	456·0	457·90	
465·3		79·70	465·1		79·85	461·7		459·8		79·85
542·0	543·50		542·3	543·80		537·6	539·65	536·1	537·75	
545·0		79·45	545·3		80·10	541·7		539·4		79·55
462·8	464·05		462·7	463·70		458·2	459·95	456·8	458·20	
465·3		79·45	464·7		79·80	461·7		459·6		79·65
542·0	543·50		542·3	543·5		537·6	540·05	536·0	537·85	
545·0		79·65	544·7		79·75	542·5		539 7		79·70
462·9	463·85		462·8	463·75		457·3	460·00	456·5	458·15	
464·8		79·85	464·7		79·60	462·7		459·8		79·60
542·7	543·70		541·9	543·35		536·6	539·5	535·8	537·75	
544·8		79·45	544·8		79·75	542·1		539·7		79·55
463·4	464·25		462·3	463·6		457·2	459·75	456·4	458·20	
465·1		79·70	464·9		79·85	462·3		460·0		79·55
542·6	543·95		541·3	543·45				535·7	537·75	
545·3		79·75	545·6					539·8		
462·8	464·20									
465·6										
Mean..... 79·64			Mean..... 79·79			Mean..... 79·90		Mean..... 79·69		

The mean value of these four series is 79·755 parts of the scale $=79{\cdot}4$ millims., which must be increased by $\frac{1}{2}$ a millim. if we are to take into account the influence of the fact that the rotation of the ring A cannot be effected in a time so small that it can be neglected in comparison with the time of oscillation of the needle. From this we obtain for α the value

$$\alpha = \frac{79{\cdot}9}{8175},$$

inasmuch as double the horizontal distance of the mirror from the scale is exactly 8175 millims.

The time of oscillation of the needle was found from 300 oscillations to be

$$t = 10''{\cdot}2818,$$

in which the part of the directive force arising from the elasticity of the thread was the 1770th part of the magnetic directive force, and hence

$$\frac{1}{1+\theta} = \frac{1770}{1771}.$$

Finally, on account of the great distance of the two rings in a room not free from iron, the time of oscillation of the same needle was compared for the position of both rings, and their ratio found to be as 2·9126 : 2·9095; from which it follows that if T′ is the terrestrial magnetism for A, T″ for B, we have

$$T' : T'' = 470 : 471.$$

These observations are sufficient for determining the resistance of the whole closed conductor; and by accurate calculation we get the value

$$w = 2166{\cdot}10.$$

§ 4. *Application of the principle of Deadening.*

Instead of using *terrestrial magnetism* to obtain an electromotive force which can be referred to an absolute measure, *bar magnetism* may be employed; in that case it is obvious that the most convenient position for the bar-magnet whose magnetism is to be used, will be in the centre of the ring formed by the closed conductor. The magnet may then either be fixed, and the ring moveable about its diameter at right angles to the magnetic axis of the bar; or inversely, the ring may be fixed and the magnet moved backwards and forwards about that diameter. In the latter case a strong *oscillating magnetic needle* may be used, suspended in the centre of the ring.

The current produced in the closed conductor by the electromotive force arising from the bar magnetism of a magnetic needle oscillating in the centre of the ring, itself reacts according to the principle of *deadening* on the oscillating needle, and produces a diminution in the amplitude of its oscillations which can be observed with great accuracy; and the *intensity* of this current may also, from these observations, be determined according to an absolute standard with great accuracy. It is then evident that the current does not need to be passed through a second ring serving as galvanometer, in order to measure the intensity of the current. Hence the whole conductor, whose resistance is to be measured, can be used to form a single ring which serves at once for indicator and multiplicator.

According to this simplification, the *observation of the arcs of oscillation of a magnetic needle oscillating in the centre of the ring* is sufficient: by their *magnitude* the strength of the electromotive force, and by their *decrease* the intensity of the current produced in the closed conductor by that electromotive force, can be determined.

In executing the observations according to this principle of *deadening*, it is of prime importance that the magnetism of the needle oscillating in the centre of the ring be very powerful; and also that the length of the needle be very small as compared with the diameter of the ring, in order that, in calculating the resistance, there shall be no necessity for an accurate knowledge of the distribution of the magnetism in the needle, the determination of which would be difficult. In the ring now solely used, which is that previously called B, and which has 303·51 millims. internal, and 374·41 millims. external diameter, and is 202 millims. in height, a magnetic needle 90 millims. long, and as strong as possible, was suspended. The experiment was commenced by *detaching* from each other the ends of the wire forming the ring. The needle was then set in oscillation, and its time of oscillation and the decrease of its amplitude, or the logarithmic decrement of this decrease, was determined according to the method given by Gauss in the 'Results of the Observations of the Magnetic Verein in the year 1837*.' Thereupon the annular conductor was *closed*, and the same observations repeated. The results of these observations are given in the following Table, in which the logarithmic decrement of the diminution of the arc of oscillation with a *closed* conductor, stands in the first column under A, the same with an open conductor stands under B, while in the third column under t is given the observed time of oscillation. The mean values are indicated underneath:—

* See Taylor's Scientific Memoirs, Part VI. Vol. II.

A.	B.	*t*.
0·028645	0·000460	9·1128
0·027955	0·000360	9·1148
0·028565	0·000380	9·1107
0·028388	0·000400	9·1128

From this we obtain, according to Brigg's system, for that part of the logarithmic decrement arising from the deadening,

$$0{\cdot}028388 - 0{\cdot}000400 = 0{\cdot}027988,$$

or according to the natural system,

$$\lambda = 0{\cdot}064445.$$

The bar magnetism of the oscillating needle M, determined from magnetometric measurements, was found, according to absolute standard as compared with the horizontal part of the earth's magnetism T,

$$\frac{M}{T} = 20733000.$$

That part of the directive force of the needle arising from the elasticity of the thread was found to be 68 times less than that arising from the magnetism, or

$$\frac{1}{1+\theta} = \frac{68}{69}.$$

For the calculation of the resistance from these observations, executed on the principle of deadening, we have the following rules.

According to the law of magnetic induction, the *electromotive force* of a small magnet oscillating in the centre of a circular conductor, whose magnetic axis makes the angle ϕ with the plane of the circle, is directly proportional to its magnetism M, to the cosine of the angle ϕ, and to the velocity of rotation $\frac{d\phi}{dt}$, and inversely proportional to the diameter of the circle r; and if M is expressed according to an absolute measure, is determined by

$$e = \frac{2\pi M}{r} \cdot \cos\phi \frac{d\phi}{dt}.$$

On the contrary, according to electro-magnetic laws the *momentum of rotation* which the induced current in the circular conductor exerts upon the small magnet oscillating in the centre

is directly proportional to the magnetism M, to the cosine of the angle ϕ, and to the intensity, and is inversely proportional to the diameter r; and if i is expressed in absolute measure, is determined by

$$D\frac{d\phi}{dt} = \frac{2\pi M}{r} \cdot i \cos \phi.$$

For small oscillations in which ϕ differs little from O, we have

$$e = \frac{2\pi M}{r} \cdot \frac{d\phi}{dt},$$

$$D\frac{d\phi}{dt} = \frac{2\pi M}{r} \cdot i.$$

If K is the inertia of the oscillating magnet, upon which the directive force MT, arising from the horizontal part of the terrestrial magnetism, acts, the equation of its motion becomes

$$O = \frac{dd\phi}{dt^2} + \frac{MT}{k}\phi + \frac{D}{k}\frac{d\phi}{dt},$$

and hence by integration,

$$\phi = p + Ae^{-\frac{Dt}{2k}} \sin (t - B) \sqrt{\left(\frac{MT}{K} - \frac{1}{4}\frac{DD}{KK}\right)}.$$

$\frac{D}{2K}$ is the logarithmic decrement on the natural system of the diminution of the amplitude of oscillation reduced to the unit of time: hence if τ is the time of oscillation under the influence of deadening,

$$\lambda = \frac{D\tau}{2K} = \frac{\pi M}{rK} \cdot \frac{dt}{d\phi} \cdot \tau i;$$

and the *intensity of the current* is

$$i = \frac{rK\lambda}{\pi M\tau} \cdot \frac{d\phi}{dt}.$$

From this we obtain for calculating the resistance,

$$w' = \frac{e}{i} = \frac{2\pi\pi MM}{rrK\lambda} \cdot \tau.$$

From the above equation for ϕ we get for the determination of the time of oscillation under the influence of the deadening,

$$\tau\sqrt{\left(\frac{MT}{K} - \frac{1}{4}\frac{DD}{KK}\right)} = \pi = \tau\sqrt{\left(\frac{MT}{K} - \frac{\lambda\lambda}{\tau\tau}\right)},$$

from which

$$M\tau = \frac{\pi\pi + \lambda\lambda}{\tau T};$$

hence $$w' = \frac{2\pi\pi}{rr} \cdot \frac{\pi\pi + \lambda\lambda}{\lambda\tau} \cdot \frac{M}{T}.$$

From this, taking into account the correction arising from the deadener as being made up of several windings, and the correction for the elasticity of the thread, we find from the above observations

$$w' = 1898 \cdot 10^8.$$

[To be continued.]

XXX. *Notices respecting New Books.*

An Elementary Treatise on Trilinear Coordinates, the Method of Reciprocal Polars, and the Theory of Projections. By the Rev. N. M. Ferrers, *M.A., Fellow and Mathematical Lecturer of Gonville and Caius College, Cambridge.* Cambridge: Macmillan and Co., 1861.

In the researches of the ancient geometers a problem presented itself to them in an almost tangible shape; the eye was a most important auxiliary to the brain; and, without questioning the truth of the old French definition, "La géométrie est une science par laquelle on raisonne droit sur des figures faites de travers," there is no doubt that a well-drawn figure would often suggest a property or method of investigation which might otherwise have escaped; and at any rate the ancients never contemplated reasoning on symbols which bore no resemblance whatever to the figure. The moderns, however, without any loss of distinctness of conception, have, by the introduction of symbols, gained important advantages. Among others, they have freed themselves from the necessity of verifying their results in every variation of case arising from a mere change of position in the data of a problem, and they have acquired an almost unlimited power of generalization.

The coordinate geometry was one of the first grand steps in this direction; but many important additions have been made since Descartes; and of late years new methods of investigation have been pursued which bid fair to carry science onwards with a speed and safety hitherto undreamt of.

The book before us makes known in a simple and intelligent manner the characteristic features of these new methods; it seems especially prepared with reference to the wants of students in the University of Cambridge, and will prove a valuable complement to the works now in use there as text-books. In his preface, the author says that his object in writing on the subject of trilinear coordinates has mainly been to place it on a basis altogether independent of the Cartesian system; but as several results of that system are assumed, as, for instance, in the definition of a conic, p. 33, and in the means of determining the centre of a conic, p. 35, it is obviously not intended to be throughout a perfectly independent work which may be studied without any previous knowledge of any other;

for a student who should attempt this would find his progress stayed at the beginning of the second chapter. This, however, does in no way detract from the merit of the book, which, we repeat, must be considered as a complement and a valuable step in advance.

The terms Trilinear coordinates, Anharmonic ratio, Involution, Reciprocal Polars, &c., have been for some years familiar terms in the studies of the University of Cambridge; and those who have read Salmon's 'Conic Sections' or Todhunter's 'Coordinate Geometry,' know the immense power they confer as a means of investigation; but we meet here with a term which to many students will be a new one, although the subject owes its existence and vitality chiefly to the labours of our own countrymen, Sylvester, Salmon, Boole, Spottiswoode, Cayley, and others. What is a determinant? Answer: Write down n rows of symbols with n symbols in each row, and enclose the whole between two vertical lines. That is a determinant. It is a conventional form of expressing in a concise manner a complicated function of these n symbols; and these same functions are so frequently recurring, not only in investigations concerning curves, but in almost every branch of mathematical inquiry, that an abridged notation for them was absolutely needed. A determinant of 5 rows, and therefore containing 25 symbols, would, if written at full length, contain 120 terms with 5 symbols in each term, *i. e.* 600 symbols instead of 25. Chapter III. of the book is devoted to a clear exposition of the simplest laws of combination of these functions, and will serve as a most useful introduction to the study of many modern scientific memoirs. We are only sorry that Mr. Ferrers does not dwell at greater length on them, and give us exact proofs of some of the remarkable results to be found in Spottiswoode, Salmon, Brioschi, Crelle's Journal, &c.

We cordially recommend this little work to those of our readers who have mastered the ordinary coordinate geometry.

XXXI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 164.]

November 22, 1860.—Major-General Sabine, R.A., Treasurer and Vice-President, in the Chair.

THE following communications were read:—

"Researches on the Phosphorus-Bases."—No. VIII. Oxide of Triethylphosphine. By A. W. Hofmann, LL.D. Received July 24, 1860.

In our former experiments *, Cahours and myself had often observed this substance, but we did not succeed in obtaining it in a state of purity fit for analysis. Nevertheless, founding our conclusion on the composition of the corresponding sulphur-compound, and having regard to the analogies presented by the corresponding terms of the arsenic- and antimony-series, we designated this body as

* Phil. Trans. 1857, p. 575.

the oxide of the phosphorus-base

$$C_6 H_{15} PO = (C_2 H_5)_3 PO\ *.$$

I have since confirmed this formula by actual analysis.

The difficulties which in our former experiments opposed the preparation of this compound in the pure state, arose entirely from the comparatively small quantity of material with which we had to work. Nothing is easier than to obtain the oxide in a state of purity, provided the available quantity of material is sufficient for distillation. In the course of a number of preparations of triethylphosphine for new experiments, a considerable quantity of the oxide had accumulated in the residues left after distilling the zinc-chloride-compound with potash. On subjecting these residues to distillation in a copper retort, a considerable quantity of the oxide passed over with the aqueous vapours, and a further quantity was obtained, as a tolerably anhydrous but strongly coloured liquid, by dry distillation of the solid cake of salts which remained after all the water had passed over. The watery distillate was evaporated on the water-bath as far as practicable, with or without addition of hydrochloric acid; and the concentrated solution was mixed with solid hydrate of potassium, which immediately separated the oxide in the form of an oily layer floating on the surface of the potash. The united products were then left in contact with solid potash for twenty-four hours and again distilled. The first portion of the distillate still contained traces of water and a thin superficial layer of triethylphosphine. As soon as the distillate solidified, the receiver was charged, and the remaining portion—about nine-tenths—collected separately as the pure product. To prevent absorption of water, the quantity required for analysis was taken during the distillation.

With reference to the properties of oxide of triethylphosphine, I may add the following statements to the description formerly given†. This substance crystallizes in beautiful needles, which, if an appreciable quantity of the fused compound be allowed to cool slowly, frequently acquire the length of several inches. I have been unable to obtain well-formed crystals; as yet I have not found a solvent from which this substance could be crystallized. It is soluble in all proportions, both in water and alcohol, and separates from these solvents on evaporation in the liquid condition, solidifying only after every trace of water or alcohol is expelled. Addition of ether to the alcoholic solution precipitates this body likewise as a liquid. The fusing-point of oxide of triethylphosphine is 44°; the point of solidification at the same temperature. It boils at 240° (corr.).

As no determination of the vapour-density of any member of the group of compounds to which oxide of triethylphosphine belongs has yet been made, it appeared to me of some interest to perform this experiment with the oxide in question. As the quantity of material at my disposal was scarcely sufficient for the determination by Dumas's method, and Gay-Lussac's was inapplicable on account of the high boiling-point of the compound, I adopted a modifica-

* H = 1; O = 16; C = 12, &c. † Phil. Trans. 1857, p. 575.

tion of the latter, consisting essentially in generating the vapour in the closed arm of a U-shaped tube immersed in a copper vessel containing heated paraffin, and calculating its volume from the weight of the mercury driven out of the other arm. Since I intend to publish a full description of this method, which promises to be very useful in certain cases, I shall here content myself with stating the results obtained in one of the experiments.

Substance	0·150 grm.
Volume of vapour	49·1 cub. cent.
Temperature (corrected)	266·6
Barometer at 0°	0·7670 metre.
Additional mercury column at 0°	0·1056 „

These numbers prove the vapour-density of oxide of triethylphosphine to be 66·30, referred to hydrogen as unity, or 4·60 referred to atmospheric air. Assuming that the molecule of oxide of triethylphosphine corresponds to 2 volumes of vapour*, the spec. grav. of its vapour $=\frac{132}{2}=67$, when referred to hydrogen, and 4·63 when referred to air. Hence we may conclude that in oxide of triethylphosphine the elements are condensed in the same manner as in the majority of thoroughly investigated organic compounds.

From the facility with which triethylphosphine is converted into the oxide by exposure to the air, even at ordinary temperatures, and the very high boiling-point of the resulting compound, in consequence of which the vapour of the latter can exert but a very slight tension at the common temperature, I am induced to think that the phosphorus-base may be used in many cases for the volumetric estimation of oxygen. When a paper ball soaked in triethylphosphine is passed up in a portion of air confined over mercury, the mercury immediately begins to rise, and continues to do so for about two hours, after which the volume becomes constant, the diminution corresponding very nearly to the proportion of oxygen in the air. To obtain very exact results, however, it would be probably necessary in every case to remove the residual vapour of triethylphosphine by means of a ball saturated with sulphuric acid.

Oxide of triethylphosphine exhibits in general but a small tendency to unite with other bodies. Nevertheless it forms crystalline compounds with iodide and bromide of zinc. I have examined more particularly the iodine-compound.

Oxide of Triethylphosphine and Iodide of Zinc.—On mixing the solutions of the two bodies, the compound separates, either as a crystalline precipitate or in oily drops which soon solidify with crystalline structure. It is easily purified by recrystallization from alcohol, when it is deposited in often well-formed monoclinic crystals containing $C_6H_{15}PO, ZnI = (C_2H_5)_3PO, ZnI$.

It is remarkable that this compound formed in presence of a large excess of hydriodic and even of hydrochloric acid.

Oxide of Triethylphosphine and Dichloride of Platinum.—No precipitate is formed on mixing the aqueous solutions of the two com-

* $H_2O = 2$ vols. vapour.

pounds, however concentrated. But on adding the anhydrous oxide to a concentrated solution of dichloride of platinum in absolute alcohol, a crystalline platinum-compound is deposited after a few moments. This compound is exceedingly soluble in water, easily soluble in alcohol, insoluble in ether. On adding ether to the alcoholic solution, the salt is precipitated, although with difficulty, in the crystalline state. The alcoholic solution, when evaporating spontaneously, yields beautiful hexagonal plates of the monoclinic system, frequently of very considerable dimensions. The crystals have the rather complex formula

$$C_{24}H_{60}P_4O_3Pt_2Cl_8 = 3[(C_2H_5)_3PO] + (C_2H_5)_3PCl_2, 2PtCl_2.$$

On mixing the concentrated solution of the oxide with trichloride of gold, a deep yellow oil is separated, which crystallizes with difficulty after considerable standing. This compound is exceedingly soluble in water and in alcohol. When the aqueous solution is heated, the gold is reduced ; the transformation which the oxide of triethylphosphine undergoes in this reaction is not examined.

Chloride of tin forms likewise an oily compound with the oxide : I have not succeeded in crystallizing this compound.

Chloride of mercury is without any action on oxide of triethylphosphine.

Oxychloride of Triethylphosphine.—On passing a current of dry hydrochloric acid through a layer of oxide of triethylphosphine which is fused in a U-shaped tube surrounded by boiling water, brilliant crystals are soon deposited. These crystals disappear, however, rapidly, the compound formed in the commencement of the reaction uniting with an excess of hydrochloric acid. The viscous liquid which ultimately remains behind, when heated loses the excess of hydrochloric acid, leaving an exceedingly deliquescent crystalline mass, very soluble in alcohol, insoluble in ether.

For analysis, the new compound was washed with absolute ether and dried over sulphuric acid *in vacuo*, either at the common temperature or at 40°. Three chlorine-determinations in specimens of different preparations, which, owing to the extraordinary avidity of this compound for moisture, exhibit greater discrepancies than are generally observed in experiments of this description, lead to the formula

$$C_{12}H_{30}P_2OCl_2 = (C_2H_5)_3PO, (C_2H_5)_3PCl_2.$$

The dichloride of triethylphosphine cannot be formed by the action of hydrochloric acid upon the oxide.

The oxychloride exhibits with other compounds the deportment of the oxide. It furnishes with dichloride of platinum the same platinum-salt which is obtained with the oxide. In a similar manner it gives with iodide of zinc the iodide of zinc-compound of the oxide previously described. Only once—under conditions not sharply enough observed at the time, and which I was afterwards unable to reproduce in repeated experiments—a compound of the oxychloride with iodide of zinc was formed. This substance, readily soluble in water and alcohol, crystallized from the latter solvent in beautiful colourless, transparent octahedra of the composition

$$C_{12}H_{30}P_2OCl_2Zn_2I_4 = (C_2H_5)_3PO, (C_2H_5)_3PCl_2, 2ZnI.$$

"Researches on the Phosphorus-Bases."—No. IX. Phospharsonium Compounds. By A. W. Hofmann, LL.D. Received July 24, 1860.

The facility with which the bromide of bromethyl-triethylphosphonium furnishes, when submitted to the action of ammonia and monamines, the extensive and well-defined group of phosphammonium-compounds, induced me to try whether similar diatomic bases containing phosphorus and arsenic might be formed by the mutual reaction between the bromethylated bromide and *monarsines*. There was no necessity for entering into a detailed examination of this class of compounds. I have, in fact, been satisfied to establish by a few characteristic numbers the existence of the phospharsonium-group.

Action of Triethylarsine on Bromide of Bromethyl-triethyl-phosphonium.

On digesting the two substances in sealed tubes at 100°, the usual phenomena are observed; the reaction being complete after the lapse of twenty-four hours. The saline mass which is formed yields with oxide of silver in the *cold*, a powerfully alkaline solution, containing the hydrated oxide of ethylene-hexethylphospharsonium,

$$C_{14}H_{36}PAsO_2 = \frac{[(C_2H_4)''(C_2H_5)_6PAs]''}{H_2}O_2.$$

It is thus obvious that the arsenic-base imitates triethylphosphine in its deportment with the brominated bromide. The two substances simply combine to form the dibromide of the phospharsonium,

$$[(C_2H_4Br)(C_2H_5)_3P]Br + (C_2H_5)_3As = \left[(C_2H_4)''\frac{(C_2H_5)_3P}{(C_2H_5)_3As}\right]''Br_2.$$

The alkaline solution of the oxide of the phospharsonium exhibits the leading characters of this class of bases; I may therefore refer to the account which I have given of the oxide of diphosphonium. The saline compounds likewise resemble those of the diphosphonium. The dichloride and the di-iodide were obtained in beautiful crystalline needles, exhibiting a marked tendency to form splendidly crystallized double compounds. I have prepared the compounds of the dichloride with chloride of tin, bromide of zinc, trichloride of gold, and lastly with dichloride of platinum. The latter compound was analysed in order to fix the composition of the series.

Platinum-salt.—The product of the reaction of triethylarsine upon the bromethylated bromide was treated with oxide of silver in the *cold*, and the alkaline solution thus obtained, saturated with hydrochloric acid and precipitated with dichloride of platinum. An exceedingly pale-yellow, apparently amorphous precipitate of diphosphonic appearance was thrown down, almost insoluble in water, but dissolving in boiling concentrated hydrochloric acid. The hydrochloric solution deposited, on cooling, beautiful orange-red crystals, resembling those of the diphosphonium-platinum-salt. The crystals, according to the measurement of Quintino Sella, belong to the tri-

metric system. The analysis of the platinum-salt led to the formula

$$C_{14}H_{34}PAsPt_2Cl_6 = \left[(C_2H_4)'' \begin{matrix}(C_2H_5)_3P \\ (C_2H_5)_3As\end{matrix}\right]''Cl_2, 2PtCl_2.$$

The phospharsonium-compounds, and more especially the hydrated oxide of the series, are far less stable than the corresponding terms of the diphosphonium- and even of the phosphammonium-series. If the product of the action of triethylarsine upon the brominated bromide be *boiled* with oxide of silver instead of being treated in the cold, not a trace of the phospharsonium-compound is obtained. The caustic solution which is formed, when saturated with hydrochloric acid and precipitated with dichloride of platinum, furnishes only the rather soluble octahedral crystals of the oxethylated triethylphosphonium-platinum-salt. The nature of this transformation is clearly exhibited when a solution of the dioxide of phosphonium is submitted to ebullition. Immediately the clear solution is rendered turbid from separated triethylarsine, which becomes perceptible, moreover, by its powerful odour, the liquid then containing the oxide of the oxethylated triethylphosphonium,

$$\left.\begin{matrix}[(C_2H_4)''(C_2H_5)_6PAs]'' \\ H_2\end{matrix}\right\}O_2 = (C_2H_5)_3As + \left.\begin{matrix}[(C_2H_5O)(C_2H_5)_3P] \\ H\end{matrix}\right\}O.$$

GEOLOGICAL SOCIETY.

[Continued from p. 166.]

May 22, 1861.—Leonard Horner, Esq., President, in the Chair.

The following communications were read :—

1. "On the Geology of a part of Western Australia." By F. T. Gregory, Esq.

The author first described the granitic and gneissose tract of the elevated table-land ranging northwards from Cape Entrecasteaux and comprising the Darling Downs. The igneous rocks and quartz-dykes were next referred to, and also the clays, sandstones, and conglomerates capping the table-land. Carboniferous, cretaceous, and pleistocene rocks were also alluded to; and some evidences of the recent elevation of the coast were brought forward. Besides specimens of rocks and minerals, the following fossils from Western Australia were exhibited: Carboniferous fossils and cannel-coal from the Irvin River; Fossils of secondary age (*Trigoniæ*, *Ammonites*, and fossil wood) from the Moresby Range; fossil wood from the Stirling Range and from the Upper Murchison River; Ventriculites in flint from Gingin, and Brown-coal from the Fitzgerald River. The author's views of the geology of the district were shown by an original map and accompanying sections.

2. "On the Zones of the Lower Lias and the *Avicula contorta* Zone." By Charles Moore, Esq., F.G.S.

Referring to a paper on this subject, by Dr. Wright, which appeared in the sixteenth volume of the Society's Journal, the author stated that details of the section at Beer-Crowcombe (near Ilminster) in Somersetshire are now more fully known than they were when

the Rev. P. B. Brodie, after having been taken to see that section by the author, communicated to Dr. Wright the notes on it that are published in the paper above referred to. In the first place, Mr. C. Moore described the characters of the Liassic beds at Ilminster, and their relations to the *Avicula contorta* beds and the Keuper as seen in passing from Ilminster through Beer-Crowcombe to Curry-Rival and North Curry,—a distance of ten miles. He then treated of the subdivisions of the Lower Lias and the true position of the "White Lias;" and stated that, although Dr. Wright had proposed the following classification—5. *Ammonites Bucklandi* zone; 6. *A. Planorbis* zone (including the White Lias and the *Ostrea* beds); and 7. *Avicula contorta* zone, yet he preferred to group them thus—5. *A. Bucklandi* zone; 6. *A. Planorbis* zone; 7. Enaliosaurian zone; 8. White Lias; 9. *Avicula contorta* zone: 8 and 9 being equivalent to the "Kössener Schichten" or "Rhætic beds" of Gümbel and other Continental geologists.

The arguments in favour of his views the author based chiefly on observations made at Beer-Crowcombe, Stoke St. Mary, Pibsbury, Long Sutton, and other places in Somersetshire; and on a critical examination of the sections at Street, Saltford, &c. as given by Dr. Wright.

The communication concluded with descriptions of upwards of sixty species of fossils belonging to the Rhætic beds of England (including their thin representatives discovered by the author in the Vallis near Frome). Twenty-eight of these species are new.

XXXII. *Intelligence and Miscellaneous Articles.*

ADDITIONAL NOTE ON THE CRYSTALS OF LAZULITE DESCRIBED IN THE AUGUST NUMBER OF THIS JOURNAL.

DEAR SIR, *To Dr. William Francis.*

SINCE the transmission of my paper on the American crystals of Klaprothine or Lazulite, I have received a communication from Professor George J. Brush of Yale College, New Haven, informing me that the crystals in question do not come from North Carolina, but from Georgia. They occur at Graves' Mountain in Lincoln County of that State. The North Carolina examples, analysed by Smith and Brush, do not appear to have been met with in crystals.

Prof. Brush also informs me that these Georgian crystals have been described and figured in a paper by Prof. Shepherd, in the American Journal of Science and Arts, vol. xxvii. (2nd series). This paper had quite escaped my notice, and I have at present no means of referring to it. I hasten, however, in apologizing for past negligence, to point out the fact of its publication. As regards the assumed Trimetric character of these crystals, my views, I may venture to observe, remain unchanged.

Trusting that you will allow this explanation an early place in the pages of the Philosophical Magazine,

I am, dear Sir,

Sault de Ste. Marie, Lake Huron, July 29, 1861.

Yours very truly,

E. J. CHAPMAN.

ON OZONE, NITROUS ACID, AND NITROGEN.

BY T. STERRY HUNT, F.R.S.

The formation of a nitrite when moist air is ozonized by means of the electric spark (the old experiment of Cavendish) or by phosphorus, was shown by Rivier and de Fellenberg, who concluded that the reactions ascribed by Schönbein to ozone were due to traces of nitrous acid. The subsequent experiments of Marignac and Andrews have, however, established that ozone is really a modification of oxygen, which Houzeau has shown to be identical with the so-called nascent oxygen, which is evolved, together with ordinary oxygen, when peroxide of barium is decomposed by sulphuric acid at ordinary temperatures. The spontaneous decomposition of a solution of permanganic acid also evolves a similar product having the characters of ozone.

Believing that the nitrous acid in the above experiments is not an accidental product of electric or catalytic action, but dependent upon the formation of active or nascent oxygen, I caused a current of air to pass through a solution of permanganate of potash mixed with sulphuric acid. The air, which had thus acquired the odour and other reactions of ozone, was then passed through a solution of potash; by which process it lost its peculiar properties, while the potash solution was found to contain a salt having the reactions of a nitrite.

As I suggested in this Journal in 1848, I conceive gaseous nitrogen to be the anhydride amide or nitryle of nitrous acid, which in contact with water might under certain circumstances generate nitrous acid and ammonia. From the instability of the compound of these two bodies, however, it becomes necessary to decompose one at the instant of its formation in order to isolate the other. Certain reducing agents which convert nitrous acid into ammonia may thus transform nitrogen (NN) into $2NH^3$. In this way I explain the action of nascent hydrogen in forming ammonia with atmospheric nitrogen in presence of oxidizing metals and alkalies. (Zinc in presence of a heated solution of potash readily reduces nitrates and nitrites with the evolution of ammonia.)

Now an agent which, instead of attacking the nitrous acid would destroy the newly formed ammonia, would permit us to isolate the nitrous acid. Houzeau has shown that nascent oxygen is such an agent, at once oxidizing ammonia with formation of nitrate (nitrite?) of ammonia; and thus when ozone (nascent oxygen) is brought in contact with moist air, both of the atoms of nitrogen in the nitryle (NN) appear in the oxidized state.

From this view it follows that the odour and most of the reactions ascribed to ozone are due to nitrous acid which is liberated by the decomposition of atmospheric nitrogen in presence of water and nascent oxygen. We have thus a key to a new theory of nitrification, and an explanation of the experiments of Clocz on the slow formation of nitrite by the action of air exempt from ammonia upon porous bodies moistened with alkaline solutions.—Silliman's *American Journal* for July 1861.

THE

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PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FOURTH SERIES.]

OCTOBER 1861.

XXXIII. *On the Cohesion-Figures of Liquids.* By CHARLES TOMLINSON, *Lecturer on Science, King's College School, London**.

[With a Plate.]

WE are accustomed to consider a solution as an example of adhesion, as when water adheres to and dissolves a salt, or mercury a metal. In such cases the adhesion is sufficiently powerful to overcome the cohesion of the solid. This process continues until the adhesion of the liquid and the cohesion of the solid counterbalance each other, and we then get what is called *saturation*. The solution of one liquid in another is also a case of adhesion overcoming cohesion. The solution of a gas or of a vapour in a liquid may also be regarded as a case of adhesion; but often accompanied by this additional phenomenon, that the particles of the gas or vapour reassume the cohesive states of their liquids. For example, if we hold a pellet of sponge saturated with sulphuric ether about half an inch over the surface of water, a portion of the vapour of the ether will be condensed upon the surface in the form of a film with a sharp, well-defined edge; and this will continue so long as the sponge is wet, but diminishing in size as the ether evaporates. So powerful is the adhesion between the water and the ether, that, if the surface of the former be dusted with lycopodium or tripoli, or any loose dry powder, the ether vapour will sweep it aside, and it will be seen, in a state of agitation, outside the edge of the ether film.

When one liquid is added to another, and solution takes place between them, there is always a breaking up of the cohesion of

* Communicated by the Author, having been read at the British Association at Manchester, September 1861.

one or other liquid: where there is no solution, there may be simply adhesion. In both cases, whether there be solution or not, one of the liquids displays the phenomena of cohesion in a characteristic manner. For example, the essential oils are but slightly soluble in water. If we place a drop of oil of lavender on the surface of water, the adhesion of the water will cause it to spread out into a film; but the cohesion of the oil immediately begins to reassert itself; the film opens in a number of places, forming long irregular arms or processes resembling the pattern assumed by wood when it has been much worm-eaten. These processes tend to gather up into separate discs or lenticules; the adhesion of the water spreads them out, the cohesion of the oil struggles to prevent this, and soon prevails; the almost immediate issue being the formation of the original drop into a number of discs with sharp, well-defined outlines and convex surfaces. The action is often so rapid, and the pattern so complicated, that it requires repeated observation to become master of all the phenomena. (See Plate IV. principal figure, and the subsidiary figures *a*, *b*, *c*, *d*.)

Now this struggle on the part of the oil of lavender to preserve its cohesion gives rise to a figure which is characteristic of the substance, and which I propose to name its *cohesion-figure*. It may be regarded as the resultant of the cohesive force of the substance, its density, and the adhesion of the surface on which it is placed. I believe that every independent liquid has its own cohesion-figure. By an independent liquid, I mean not a solution; for in the solutions of solids and liquids cohesion has been already overcome.

The cohesion-figures of liquids can be conveniently studied by gently placing on the surface of water, of mercury, &c. a drop of the substance in question, which we will suppose exerts no chemical action on the receiving surface. Now the cohesion-figures of liquids will be more or less permanent in the inverse ratio of the solubility of the substance. A drop of one of the fixed oils placed on the surface of water will spread out into a film, which is characteristic of the substance, and may last some minutes or even hours, according to the degree of force with which cohesion reasserts itself. A drop of one of the essential oils will also give a characteristic film or cohesion-figure which may change every moment from evaporation and display some beautiful effects of colour; but all these phenomena will be characteristic of the substance in question, and will enable it to be recognized. A drop of a substance like creosote, which is slightly soluble in water, may continue five minutes; a drop of ether or of alcohol may last only a fraction of a second; but whether the time be long or short, these figures are typical of the

substances that produce them; and so sensitive are they to any variations in the conditions under which they are produced, that a slight alteration in one of those conditions leads to a marked change in the cohesion-figure. Thus the cohesion-figure of wood-spirit on water is very different from what it is on mercury, since the surface attraction or adhesion of mercury is very different from that of water.

Now let us examine the cohesion-figure of a liquid that is sparingly soluble in water, such as creosote. If we deliver a drop of this substance from the end of a glass rod to the surface of one ounce of water, we may witness a struggle between cohesion and adhesion that will last about five minutes. The creosote sails about on the surface of the water in a state of considerable agitation, discharging a number of small globules on all sides, which, in their turn, are greatly agitated; they rotate and disappear, leaving behind them a thin silvery film. Meanwhile the parent globule diminishes in size, but preserves all the characteristics of its cohesion-figure, until at length it disappears in the form of a film. If a second drop of creosote be now placed on the water, its behaviour will resemble that of the first in a mitigated form; it will be much less energetic, and will last a much longer time before it disappears in the form of a film. A third drop will remain on the surface in the form of a double convex lens sharply defined, showing that the cohesion of the creosote exactly balances the adhesion of the water; or, in other words, that saturation has been attained. If we now add more water, or increase its solvent power by the addition of a drop of acetic acid, the action will set in again, and the lens will change into the cohesion-figure. (See Plate.)

The following comparative experiment was made with fresh colourless creosote (Morson's) in two exactly similar shallow glasses, one containing one ounce, and the other two ounces of New River water. In such water adhesion is diminished by its mineral contents. In distilled water the phenomena are the same, but the time is diminished, a drop of creosote disappearing in five minutes instead of seven:—

Glass No. 1.—1 *oz. water.*	min.	*Glass No.* 2.—2 *ozs. water.*	min.
First drop of creosote disappeared in.....	7	First drop of creosote disappeared in.....	7
Second drop disappeared in.. But two or three minute specks of creosote remained, moving in circular orbits in the film.	20	Second drop disappeared in.. The disc was flatter and more vigorous than in No. 1; towards the end the disc broke up into separate portions, which rotated with immense rapidity and disappeared.	12½

Glass No. 1.—1 *oz. water.*

Third drop had not disappeared after..... } min. 135

When this drop was first placed on the water, it repelled the film, and the crispations set in although sluggishly. No vollies of small globules. The edge like that of window glass. After 1 minute it became still. After 2 minutes lenticular, slowly sailing about, with occasional jerking of the edge. After 7 minutes, slowly revolving on vertical axis. After 13 minutes, slowly sailing about. After 25 minutes, a very convex lenticule. After 60 minutes, at rest and slowly evaporating.

Glass No. 2.—2 *ozs. water.*

Third drop disappeared in about ..... } min. 25

The disc active and vigorous, and of good figure, with vollies of minute globules. After 20 minutes, broke up into three portions, two of which were active; then one split into three or four, which were scattered to a distance; then all still: crispations slowly resumed, and after 25 minutes only a few globules, scarcely visible, remained.

Fourth drop had not disappeared after ..... } min. 110

The drop was active for a few minutes, then subsided into a well-shaped lenticule, which slowly disappeared by evaporation.

In connexion with creosote, the cohesion-figure of carbolic acid is interesting. (See Plate.) It is an exaggerated form of the figure of creosote; the water seems to tear it to pieces; the crispations are amazingly active, and the disc quickly breaks up and disappears. Indeed, while a drop of creosote will endure five minutes in an ounce of distilled water, a drop of carbolic acid will last only a few seconds in the same quantity of water. The cohesion-figure is, however, quite characteristic of the substance, and cannot be for a moment mistaken for any other substance that I have examined.

In cases of this kind, where the conditions are different, we get different cohesion-figures. It has already been stated that, by changing the receiving surface, as by substituting mercury for water, we get a new figure from the same liquid. So, also, if we change the character of the liquid, we vary the figure. The figure given by the unwashed sulphuric ether of the shops is very different from that afforded by rectified ether. Let us take up a quantity of the former in a dropping tube, and gently deliver it, drop by drop, to the surface of about 2 ozs. of water in a clean foot-glass. The very act of gently placing a drop of ether on water leads to the formation of a disc of condensed vapour, just as in pouring ether from a bottle we must first pour a quantity of vapour. As the drop of ether is hanging over the water, it forms a well-defined circular disc or film of condensed ether-vapour on the surface of the water immediately below the drop of ether. But as soon as the drop is delivered to the water, it combines with this disc, and spreads into another disc to the utmost limit of its cohesion: it forms, in fact, a circular or centrifugal wave of such extent that there is not matter enough to prevent the centre from opening and following the general im-

pulse outwards. We thus get a perforated disc: the disc itself is in rapid motion and agitation, but the water seen through the central perforation is tranquil. Both the outer and the inner edges of this disc are perfectly sharp and well defined. The cohesive force of the ether prevents it from breaking up, and even produces a rebound: the disc closes in upon itself, becomes smaller and smaller, still preserving its central perforation and well-defined form, until at length it vanishes under the influence of evaporation, adhesion of the water, and probably electrical action; or, as is generally the case, the attraction of the sides of the vessel causes the mobile body to disappear by dashing up against the glass. If two or three drops be allowed to fall in quick succession, the perforated discs become partially superposed, but still preserve their distinctive features. (See figs. *a*, *b*, *c* in the sulphuric-ether figure.)

On placing a drop of rectified ether on the surface of water, it is evident that it has a much stronger cohesion than when adulterated with alcohol, or rather the water has a less adhesion for it. The drop of ether becomes lenticular, and in doing so discharges from all sides a portion of its substance, which assumes the form of a tolerably smooth flat ring: this in its turn discharges a portion of ether into the water, which, in seizing it, produces a troubled motion. Hence we have the true ether figure, consisting of a central lenticule, surrounded by a nearly smooth flat ring with radiated markings, and this by an agitated ring with curved markings, as if minute globules of the liquid were diffusing. I have attempted, with the assistance of an artist, to represent this in the Plate. It is tolerably accurate; but I need hardly suggest that a chemist's eye retains such figures better than an artist's; for to the one they are expressions of natural truths—additional exponents, in fact, of those endless properties which he endeavours to frame into laws; while to the artist these things are mere forms—if beautiful, so much the better—but still only forms, containing no latent truths.

I may also remark that, in order to get the typical form of each substance, certain precautions are necessary. The water may be distilled; but this is not absolutely necessary, provided it be chemically clean. It should be in sufficient quantity to prevent its becoming quickly saturated; for as the water approaches saturation, the figure becomes slightly modified, although it presents the no small advantage of greater persistence. The ether figure will remain for about a second in a nearly saturated solution. But what is in many cases absolutely necessary to success is, that the glass containing the water be quite clean; it should be purified from the organic film which covers most matter exposed to the air, by washing it in strong

sulphuric acid, and then in a solution of caustic potash. If, after this, the water completely wet the glass, it may be rinsed and used. It had better not be wiped. Having been once washed with sulphuric acid, it will serve for a great number of experiments, provided it be washed in a weak solution of caustic potash after each experiment, and rinsed with clean water. The substance of which the figure is to be determined must, as already remarked, be pure: the figure given by pure washed ether becomes changed into the perforated discs by the addition of a few drops of absolute alcohol to a small quantity of the washed ether; and the unwashed ether, if exposed in an open vessel for a few minutes, will throw off its ethereal portion, and the cohesion-figure will quickly pass into the alcohol figure. It is quite remarkable how rapidly this change takes place. I had poured some unwashed ether into a test-glass, from which I fed the dropping-tube, and in about ten minutes the ether figure was completely superseded by the alcohol one. I do not pretend to say that all the substances made use of in this inquiry are pure. I have taken pains to procure them from the best sources, such as the manufacturers themselves; but some of the essential oils, for example, are prepared on the Continent, and may possibly not be quite pure.

There is not so marked a difference between the cohesion-figures produced by spirits of wine and absolute alcohol as between unwashed and washed ether: nor is this to be wondered at, seeing that spirits of wine only differ from absolute alcohol in having already received a portion of the water which the absolute alcohol takes up in forming its peculiar figure on water. The figure of spirits of wine consists of a central disc with a foliated outline surrounded by a tolerably smooth disc. The figure of absolute alcohol (Pl. IV.) has the central disc more minutely foliated than in the former case, and it has a greater tendency to a stellar arrangement.

Without further multiplying these examples, I may once more recur to the law on which they seem to rest—viz. that each figure is the resultant of the cohesive force of the liquid, its density, and the adhesion of the receiving surface. If this be true, it follows that two liquids although of very different chemical character, yet being of the same density, similarly cohesive, whether viscid or fluid, and the adhesion of the receiving surface being the same (*i. e.* having the same degree of solubility), we get precisely the same cohesion-figures for both liquids. Now creosote and oil of cloves are chemically two very dissimilar liquids. It is true that both are hydro-carbons, and that each consists of two distinct bodies; but their points of difference are numerous and important. Nevertheless their physical resem-

blances are striking: they are of about the same density (*i. e.* a little heavier than water); they are about equal in liquidity or cohesion, and they are both sparingly soluble in water. Now the cohesion-figures of these two substances are so much alike that a casual observer would declare them to be identical. (See Plate.) There are the same crispations in the oil of cloves as in the drop of creosote; the same flattened, indented, waving, agitated outline; the same sailing about; a similar film, and the same repulsion of the film; the same whirling off of small globules, and the rapid rotation and disappearance of those small bodies. Like the creosote, too, the second drop of oil of cloves reproduces the phenomena of the first in a mitigated form, and is much longer in disappearing. But now for the differences. The film formed by oil of cloves is more like smoke, more dense, persistent, and plicated than that of creosote; and being constantly driven about by the parent disc, it becomes powdery, like fine flour, on the surface of the water. In the midst of this film, the parent globule will sometimes remain for several minutes, keeping a clear space of considerable extent all around it, pulsating in a regular manner, and flashing out lines which are visible only by the motions of the water. But the most considerable difference between oil of cloves and creosote is in their respective duration. We have seen that 2 ozs. of New River water will dissolve three drops of creosote; the same quantity of river water will not dissolve so much as two drops of oil of cloves. After the first drop has disappeared, a second will be lively at intervals during nearly an hour, but after twenty-four hours some small lenticules of the oil will remain on the water. Hence oil of cloves has only about half the solubility of creosote in water—only half the adhesion, in fact; but being denser than water, it tends to sink, and thus appears to be more adhesive than it really is. An interesting result may also be obtained with ol. pimentæ, which is a little heavier than water (1·021 to 1·044). It is much more sluggish than creosote and oil of cloves, but exhibits similar phenomena on a small scale. If, however, the water be heated to about 110° F., we get a large crispating figure of great beauty. There are special characters about it which I do not stop to describe.

Should any one wish to repeat these observations on oil of cloves, he may have some difficulty in doing so on account of the difficulty of obtaining the pure oil. The oil of the shops is commonly adulterated with the cheaper oils, such as oil of olives or of almonds, or of turpentine; and I have ascertained that a single drop of oil of olives to twenty drops of oil of cloves is sufficient to prevent the formation of the cohesion-figure, and the display of those curious and interesting motions of the pure oil. Even in cases where the oil is not adulterated, the fixation

of oxygen, simple or ozonized, may prevent the display of these characteristic phenomena; but at the same time it will introduce other results which are equally characteristic.

Oil of turpentine is also used as an adulterant of the essential oils. Its presence can be detected by the brilliant iridescent colours which it imparts to films that are otherwise colourless. It also makes many oils more limpid, and thus renders them more active in the display of their peculiar phenomena. Oil of turpentine alone gives a delicate film with iridescent rings and an outer border of minute globules, with bosses within the edge: these flatten into discs surrounded by small dots. Iridescent colours now set in and cover the film. Suddenly the whole film opens into holes, which, in the midst of the colours, have a beautiful effect. The film slowly disappears, leaving an outline lace-pattern which lasts for hours.

Now when oil of lavender is adulterated with turpentine in the proportion of 5 to 1, the film spreads with a brilliant display of colour, which is characteristic of the turpentine, the lavender being colourless; at the same time the peculiar worm-eaten pattern of lavender is more minute, and its action much more rapid than in the case of the pure oil. By increasing the proportion of the turpentine, the characteristics of the latter film override those of the former.

Now this brings me to speak of the use to which these cohesion-figures may be applied in detecting adulteration. It is perfectly easy to distinguish unwashed ether from rectified ether, alcohol from spirits of wine, &c., by their respective cohesion-figures. It is also equally easy to name a varnish, a fixed, or an essential oil, from the characters of the film which a drop of each substance forms on water. Having become acquainted with the characters of each film, it is not difficult to detect the films formed by mixtures, and even to name the component parts of a mixture. For example, oil of cinnamon is now worth about 5*s* per oz., so that there is an inducement to adulterate it. The readiest means of adulteration is with oil of olives or oil of sweet almonds. To be able to detect the adulteration, we must become acquainted with the characters of the films of all three oils. Now, to begin with oil of cinnamon:—As soon as a drop of this substance is delivered to the surface of the water, it spreads out into a film, but the more fluid portion of the oil (the eleopten) precedes the film in radial lines of minute globules, and these form an outer boundary line of detached spots to the film. The film itself even on a large surface of water is not more than about an inch in diameter; it is of a beautiful delicate structure and silvery reflection; its edge is well defined, and it has small bosses just within it. Almost immediately after its

formation, holes open near the edge, starting into existence and altering rapidly, and the film separates into a kind of network and two or three well-defined flat discs.

When a drop of oil of olives is placed on water, the first thing that strikes the eye is a beautiful widening rainbow, which seems to deposit the film and then disappear. The film itself is colourless, and it has an indented edge displaying a very light and elegant kind of lace-pattern, similar to what, I believe, is called *guipure*, in which a raised thread traces the outline. (Plate IV.) After a few minutes the pattern vanishes, and the oil collects into an irregular trail with ragged edges, surrounded by numerous small globules.

A drop of oil of almonds on water spreads into a large film with a beautiful lace-like edge, which soon disappears by the holes opening into each other. (See Plate.) The edge separates from the parent film, and forms small lenticules outside it. The edge of the film appears a little raised; the holes in it continue to open and widen, and the detached pieces shrink up into lenticules; and in a few minutes the parent film has diminished to the size of a shilling, surrounded by a number of lenticules of various degrees of smallness.

Now when olive oil is used to adulterate oil of cinnamon, its presence may be detected by some of the characters which the oil-of-olives film exhibits alone, and especially by its iridescence. I added one drop of oil of olives to ten drops of oil of cinnamon; and the film formed by one drop of the mixture on water exhibited the following characters:—1st. A display of beautiful iridescent rings, which shrank into angular masses and so disappeared. 2nd. A considerable portion of the film gathered itself up into a central disc about the size of a wafer. 3rd. This disc was surrounded by a delicate perforated silvery film which quickly evaporated, leaving some minute lenticules which became fringed with a kind of frill. And then, 5th. These lenticules exhibited minute systems of iridescent rings. These iridescent effects at the commencement and close of the observation do not belong to oil of cinnamon, but are characteristic of oil of olives. These and the other phenomena are sufficient to detect the presence of a small portion of oil of olives in oil of cinnamon. By increasing the proportion of the adulterating oil, the properties of the oil of olives are displayed more strikingly.

When one drop of oil of almonds is added to ten drops of oil of cinnamon, and one drop of the adulterated oil is placed on water, there is a shooting out of minute globules in radial lines, which is characteristic of oil of cinnamon; a delicate film is formed with holes in the edge which close and open again, and in a few minutes cohesion gathers up the film in the form of

long, ragged, irregular, oily-looking smears—an effect which is characteristic of oil of almonds.

Alcohol is also used to adulterate the essential oils: its presence can be detected by phenomena which vary with the proportions used. There are also special phenomena with each oil, that would take a long time to describe.

The films formed by such common oils as sperm and colza are also characteristic. Sperm oil forms a smooth large film, which occupies the whole surface and is accompanied by iridescent rings, which disappear when the film is formed. Minute and nicely perforated holes open in the film, and after a short time long, thin, narrow cracks open in it, darting out from the holes and often connecting them together like beads on a thread. These cracks are characteristic of sperm oil. (See Plate.)

Colza oil forms a large smooth film, accompanied by iridescent rings, which immediately disappear. Minute holes open at the edge at intervals, three or four together, sharp and clean as if punched. Similar distinct perforations are also formed in other parts of the film; and these widen and thicken at the edges until the surface is covered with a kind of honeycomb-pattern, the holes pressing together in twos and threes. The characteristic feature of the colza film is to be found in these large holes with thickened edges grouped together and opening into each other. After about an hour the film becomes whitish and greasy-looking, and the holes are surrounded by dark rings. It may be remarked, however, that an increase of temperature quickens and exalts the phenomena of this and other films. Thus the effects are more numerous and more quickly brought about on a fine warm day than on a dull and cloudy one. The films should also be formed on a given fixed area of water, or the film of the same liquid may vary in thickness at different times and thus disturb the phenomena. I have found a conical foot-glass nearly 4 inches in diameter at the mouth, nearly filled with water, answer well.

A mixture of sperm and colza in various proportions forms a good film, in which may be recognized the cracks of the sperm and the peculiar holes of the colza. I think it would be easy for any one to detect the mixture of these two oils by the character of the film. (See Plate.)

I have thus briefly indicated the mode of obtaining these cohesion-figures, and their value in determining the nature and purity of various liquids in common use and which are liable to adulteration, such as sulphuric ether, the essential and fixed oils, &c. By simply noticing the cohesion-figure of sulphuric ether for example, we can decide whether it contains alcohol or not. I believe it would be easy in many cases to decide by this mode

whether liquids professedly delivered according to sample had been tampered with between the delivery of the sample and the goods.

A considerable amount of labour requires to be expended on these cohesion-figures before the subject can be said to be ripe for extended practical application; and this labour, if health and opportunity be granted me, I intend to bestow on this most interesting subject. It will be necessary to procure many specimens from different markets of the pure liquid, to take the cohesion-figure of each sample many times under varying temperatures and hygrometric conditions of the air, to make drawings of these figures, and to decide after repeated trials what is the characteristic feature of each liquid as constantly exhibited in its cohesion-figure. The next step will be to ascertain the characteristics of mixtures of certain liquids made with a view to adulteration, and by means of cohesion-figures to enable the observer to state not only whether a costly liquid be adulterated, but what it is adulterated with, and the relative proportions of the adulterated mixture. I have done all this for a moderate number of liquids, but very much remains to be done before the method can be considered as complete. There are also certain scientific questions to be answered, such as the relations between the figures of isomeric liquids.

I am aware that some of the phenomena of films on the surface of water have, on a few occasions, attracted the attention of philosophers; but no one, so far as I am informed, has considered films to differ from each other, or to be characteristic of the substances which produce them. The behaviour of films has generally had reference, in the labours of others, to some point irrespective of the films themselves—as in the repulsion-theory of B. Prevost* and the epipolic theory of Dutrochet*.

It was many years ago observed by Ermann† that a drop of sulphuric acid deposited on the clean surface of pure mercury spreads out into a film. M. Dutrochet finds that many liquids also form films on the surfaces of glass, metal, &c., provided they be chemically clean; and he attributes all these phenomena "au développement subit et toujours de courte durée de la force épipolique centrifuge." It seems to me (with great submission to so distinguished an observer as the discoverer of endosmose and exosmose) that many of the phenomena described as the effects of the epipolic force are simple results of cohesion and adhesion. M. Dutrochet repeatedly states that his phenomena cannot be produced unless the surfaces be absolutely clean. That, I am quite sure, is a correct observation; but the impurities, according

* Quoted in M. Dutrochet's work entitled *Recherches physiques sur la Force Epipolique*, Paris, 1842, and 2nd part, 1843.

† *Annales de Physique de Gilbert*, vol. xxxii.

to my view, do not act in preventing the exhibition of a new force, but simply by preventing adhesion. Many of the phenomena of cohesion-figures I have been unable to produce away from home in vessels which have been cleaned and wiped in the usual manner; but they have succeeded perfectly when the glasses were washed in a weak solution of caustic potash and rinsed in clean water.

I have taken advantage of Ermann's observation to obtain on mercury the cohesion-figures of sulphuric acid and other substances which act chemically when brought into contact with water. In the case of sulphuric acid, the drop spreads instantly and covers the surface of the mercury; but cohesion immediately begins to reassert its claims, and forms the acid near the edge into large flat bosses, each of which becomes a centre of action; minute globules pass in and out of it; similar small globules also move to and fro over the rounded edge of the mercury. After a few minutes all action ceases: the film contracts with a smooth surface and a well-defined edge. A drop of alcohol, or of ether held over the sulphuric acid film when at its widest, gathers it up in an instant into a small disc. The reason for this is, that there is a much stronger adhesion between the acid and the vapour of ether or alcohol, than between the acid and the mercury.

It was observed by Dutrochet that a drop of water placed on mercury remains globular, an effect which he explains according to his epipolic theory. I explain it by the cohesion of the water being stronger than the adhesion of the mercury; and I imagine that the absorption of water by the sulphuric acid film lessens the adhesion of the mercury, and enables cohesion to reassert its claims with more effect. If the vessel be covered up, the diffused sulphuric acid film is much more persistent. The superior cohesive force of water is also shown by placing a drop of it on the sulphuric acid film. It does not spread, but remains in a very convex lenticular state, at the same time repelling, apparently, the sulphuric acid all around it, so that the lenticule of water remains on a dry disc of mercury. These effects I attribute to the stronger adhesion between the acid and the mercury than between water and mercury. The water does not repel the acid, as Dutrochet supposed, but simply absorbs a portion around it, sufficient to allow it to rest on the mercury, and to prevent all further action of the acid.

The liquids whose cohesion-figures on water have been determined, present, of course, different figures on mercury, because one of the conditions in the production of these figures (viz. the adhesion) is no longer the same. Thus wood spirit, which on water forms a figure something like that of alcohol, produces on

mercury a lens which flattens with a well-defined edge; then a rapid motion sets in from the edge and spreads all over the surface; bosses form and disappear; the film becomes divided into two or three parts by lines, but without separating; the agitation ceases; the film spreads more and more; but at a certain point cohesion begins to reassert itself, and the film gradually contracts and at length becomes a perfect circular disc.

King's College, London,
26th July, 1861.

XXXIV. *On the Measurement of Electric Resistance according to an absolute Standard.* By Wilhelm Weber.

[Concluded from p. 240.]

§ 5. *Comparison of the Resistance determined according to absolute measure with Jacobi's Standard of Resistance.*

To compare the resistance of two conductors, there are different methods which need no explanation. The resistances considered in the preceding paragraphs have been compared according to the method examined in this memoir, and it has been found that

$$w : w' = 1138 : 1000.$$

If the first resistance be reduced to the second according to this proportion, we obtain

$$w' = \frac{1000}{1138} w = 1903 \cdot 10^8,$$

while the direct determination in the preceding paragraph gave

$$w' = 1898 \cdot 10^8.$$

From both these closely agreeing values, determined according to entirely different methods, the number $19 \cdot 10^8$ will in future be assumed as the mean value of this resistance.

Jacobi has dwelt on the importance of introducing a definite measure for resistance to be accepted by all physicists, especially at the present time, when so many voltaic investigations are being made with the most varied instruments, the comparison of which is often of great importance. For this purpose he has proposed as a *standard measure* a copper wire, which he has sent to several physicists who are engaged with voltaic measurements, and has requested them to compare this standard with theirs, and for the future to give their measurements in this measure.

This standard is a copper wire $7169\frac{3}{4}$ millims. in length, and $\frac{2}{3}$ millim. in thickness, which weighs $2249\frac{5}{10}$ milligrammes.

The standard introduced by Jacobi, which, it is to be hoped, will find general acceptance, is by no means supplanted by the

absolute measure here discussed; for it is not possible to compare every resistance directly according to this measure, while every resistance can be directly compared with Jacobi's standard. But considering the importance which absolute determinations of measure have in many investigations, it is desirable to be able to reduce all the values, made according to Jacobi's standard, to an absolute measure, which can be easily effected by comparing the resistance determined as above according to an absolute measure with the resistance of Jacobi's standard.

Such a comparison has been made; and it has been found that the two resistances are nearly as 32 : 10, or, more accurately, as 19000 : 5980. But as the first resistance has been found in absolute measure to represent 19000 million units, Jacobi's standard represents 5980 million units; or the resistance determined according to Jacobi's measure can be reduced to absolute measure by multiplication by 6 milliards. By this determination it would be possible to reproduce approximately Jacobi's standard, even if it were lost.

§ 6. *On the value of the constants found by Kirchhoff, on which the intensity of induced electric currents depends.*

The *induction-constant* which Neumann, in his development of the mathematical laws of induced electric currents, calls ϵ, has the following meaning. If W be the absolute unit of measure proposed as above for electric resistances, and W′ that measure of resistance which is actually used; if, further, C be the measure of velocity which forms the basis in establishing the above absolute measure (1 millimetre in a second); if, on the contrary, C′ be the measure of velocity actually used in measuring the induced motions and actions of the induced currents (1 Prussian inch = 26·154 millims. in a second, according to Kirchhoff), we have

$$\epsilon = 2\frac{C'W}{CW'}$$

It follows from this, that if the value of this induction-constant is once determined, any resistance given according to the measure chosen can be referred to an absolute measure.

In the determination of the induction-constant ϵ given by Kirchhoff in the seventy-sixth volume of Poggendorff's *Annalen*, the resistance of a *copper wire* has been chosen as a standard, the length of which was 1 Prussian inch = 26·154 millims., and the section 1 Prussian square inch = 684 square millims. Here unfortunately there is no determinate measure of resistance; for different pieces of copper of the same dimensions have different resistance; and it follows, therefore, that the value of the induc-

tion-constant ϵ is left undetermined within the limits of that variability of the resistance of copper. Kirchhoff himself says, "Since the conductibility of copper varies within certain limits, in giving the value of ϵ, only a limited accuracy is of interest." Kirchhoff wished to give only an approximate value of ϵ, which would be sufficient for his purpose; and he was the more content therewith because the methods and instruments which he used would scarcely have permitted a better determination of ϵ if he proposed a perfectly definite measure of resistance.

The interest which an accurate determination of the value ϵ has, is lost in consequence of that uncertainty in the choice of the measure of resistance; and it is important to restore it by the removal of that uncertainty. This may be accomplished by keeping, not to *copper in general*, but to the piece of copper actually used by Kirchhoff in his investigations, and by choosing the resistance of a wire of this copper 26·154 millims. in length, and with a section of 684 square millims. as a measure of resistance. It is thus only necessary to reduce the result found by Kirchhoff, as well as the measures made therewith or referred thereto, to the measure thus accurately determined in this manner. Kirchhoff took one Prussian inch in a second as a measure of velocity, and found in this way

$$\epsilon = \frac{1}{192};$$

from which it follows (since $C' = 26{\cdot}154\ C$) that that resistance which amounts to 52·308 units of the above absolute measure is the $\frac{1}{192}$ of the resistance of a wire of Kirchhoff's copper the length of which is 26·154 millims. and the section 584 square millims.; in other words, that the measure of resistance chosen by Kirchhoff is 10043 times that of the above absolute measure.

Although this value of ϵ can only be considered as approximative, it is interesting to compare it with other values which have been found by entirely different methods and with different instruments, because an examination of the various natural laws brought thereby into operation is obtained. Kirchhoff's measurements refer to currents produced by *voltaic induction*, and hence in his case it is the laws of voltaic induction which have been used in determining the value of ϵ. My measurements, on the contrary, refer to currents produced by *magnetic induction*, and hence in this case it is the laws of magnetic induction which lead to the value of ϵ.

First of all, the value of ϵ shall be given which is obtained from my measurements. It is clear that the value of ϵ can be determined from these measurements, if only the resistance of

Kirchhoff's copper wire is compared with the resistance of Jacobi's standard. I have made that comparison by means of the wire which Kirchhoff kindly sent to me, and can here give the result of the comparison: it is as follows.

A piece of Kirchhoff's wire which was 13·573 Prussian inches in length and 0·4061 square line in section, had a resistance which was to the resistance of Jacobi's standard as

$$1 : 106.$$

From this we get the relation of the resistance of the measure chosen by Kirchhoff to that of Jacobi's standard as

$$1 : 106 \times 13{\cdot}573 \times \frac{144}{0{\cdot}4061}.$$

If J be the resistance of Jacobi's standard, and W′ that of Kirchhoff's, we have

$$\frac{J}{W'} = 510180.$$

Now the resistance of Jacobi's standard is equal to 5980 million units of the absolute measure found above; hence, if W be the absolute resistance,

$$\frac{J}{W} = 5980000000;$$

hence

$$\frac{W'}{W} = 11720.$$

But now

$$\frac{C'}{C} = 26{\cdot}154;$$

hence

$$\epsilon = 2\frac{C'W}{CW'} = \frac{J}{224};$$

that is, one-seventh less than Kirchhoff had found. A closer agreement was not to be expected, inasmuch as only an approximate value was claimed for Kirchhoff's statement.

I may give here a determination *of the specific resistance of the different kinds of copper* which have been used for Jacobi's standard, for Kirchhoff's wire, and for the damper which I used.

The *specific resistance* of a body is usually given according to an absolute unit by taking for this unit the specific resistance of a body whose absolute resistance with a length = 1 and a section = 1 is equal to the fixed measure of resistance. But the determination of specific resistance according to this unit meets with a practical difficulty in the accurate measurement of the

section, especially in fine wires, and hence, to obviate this difficulty, Kirchhoff has indirectly ascertained the section of the wire by determining its absolute and specific gravity.

Now the determination of specific resistance according to this unit, presupposes that the resistance of a wire whose length remains unchanged, but the thickness of which is increased or diminished, varies inversely as the section. This has not, however, been proved, and, with the small alterations of section which are produced by pressure, can scarcely be proved. There is just as much reason for assuming that, if the mass and the length of the wire remain unchanged, the resistance does not alter even with a changing section. On this assumption the absolute unit would have to be fixed in another way than as being the specific resistance of a body whose absolute resistance for the length $=1$ and for a mass $=1$ is equal to the fixed measure of resistance. According to this, the specific resistance of a body would be determined by multiplying the resistance of a wire formed of that substance expressed according to the fixed measure of resistance by its mass, and dividing by the square of its length.

The specific resistances of the wires used by Jacobi, Kirchhoff, and myself will be determined according to the unit thus fixed; for apart from the above considerations, this determination is in any case the most applicable and capable of execution.

The following Table exhibits the results of these determinations:—

Quality of copper in	Length in millimetres.	Mass in milligrammes.	Resistance in absolute measure.	Specific resistance.	ϵ.
Jacobi's wire ...	7620	22435	5980000000	2310000	$\frac{1}{270}$
Kirchhoff's wire	355	4278	58500000	1916000	$\frac{1}{224}$
Weber's wire ...	3946000	152890000	190000000000	1865600	$\frac{1}{218}$

It will be seen that there is only a small difference between my copper and Kirchhoff's; while the difference in the case of Jacobi's is far more considerable, as the latter possesses a far smaller conductibility. In the supposition that Jacobi may have used galvanoplastic copper for his standard, I examined a wire of that material which I procured through the kindness of Professor Schelibach in Berlin, and found the following result, which proves, contrary to the above supposition, that galvanoplastic copper is a somewhat better conductor.

Wire of galvanoplastic copper.	Length in millimetres.	Mass in milligrammes.	Resistance in absolute measure.	Specific resistance.	ϵ.
	12780	221295	1243000000	1684000	$\frac{1}{159}$

In the last column here and in the upper Table are given the different values of ϵ which were obtained for the Neumann's induction-constant by adhering to the measure chosen by Kirchhoff, but using the different kinds of copper which have been mentioned. Adhering, however, to the absolute measure fixed as above, $C' = C$, $W' = W$, and ϵ has always the value 2.

§ 7. *On the constants of the electric laws which depend on the choice of measures.*

The law of induced currents propounded by Neumann represents the intensity of these currents as dependent on a constant the value of which must be determined from the measures according to which the magnitudes taken into consideration are to be determined. This constant Neumann has called the *induction-constant*. Such a constant occurs in the general expression of any natural law which states how one magnitude is determined by another. I may here give a summary of these constants for all the fundamental laws which refer to *electromotive force*, *intensity*, and *electric resistance*. Each of these laws represents the desired magnitude as an expression of other measurable magnitudes, which has a constant as a factor the value of which is to be determined from the measures chosen.

1. The fundamental law of the voltaic circuit represents the intensity of the current i as an expression of the electromotive force e, and of the resistance w; for, if the constant whose value is to be determined is called α,

$$i = \alpha \cdot \frac{e}{w}.$$

This constant α has the following meaning. If J, E, W are the absolute measures fixed as above for intensities, electromotive forces, and resistance; and if J', E', W' are the measures actually used, we have

$$\alpha = \frac{JEW}{J'E'W'}.$$

Hence using the absolute measure itself, $\alpha = 1$.

2. The fundamental law of electro-magnetism represents the electromotive force F as an expression of the quantity of magnetic fluid μ, of the length ds, and of the intensity i of the element, of their distance from one another r, and of a number which is given by the angle ϕ which r makes with ds; that is, if the constant whose value is to be determined from the measures chosen is β,

$$F = \beta \cdot \frac{\mu i ds}{rr} \sin \phi.$$

The constant β has the following signification:—If P is the

absolute unit of measure of the momentum of rotation (the product of a millimetre into that force which in one second imparts to the mass a milligramme, the absolute unit of measure of velocity), if M is the absolute unit of measure of the magnetic fluid, and J is the absolute measure for intensities; if, further, P′, M′, and J′ are the measures actually used,

$$\beta = \frac{PM'J'}{P'MJ};$$

consequently, using the absolute measure, $\beta = 1$.

3. Ampère's fundamental law of electrodynamics represents the electrodynamic force of attraction F as an expression of the intensities of two elements i and i', and of a number which is fixed by the relations of the lengths of the two elements to their distance $\frac{ds}{r}, \frac{ds'}{r}$; and by the three angles ϵ, θ, θ', which ds and ds' form with one another and with r; that is, if the constant whose value is to be determined from the given measures is designated by γ,

$$F = \gamma \cdot ii' \cdot \frac{ds\, ds'}{rr}\left(\cos\epsilon - \frac{3}{2}\cos\theta\cos\theta'\right).$$

The constant γ has the following signification:—If F is the absolute measure of force (that force which in a second imparts to the mass of a milligramme a velocity of a millimetre in a second), if J is the absolute measures for intensities, and F′, J′ the measures actually used, we get

$$\gamma = \frac{FJ'J'}{F'JJ};$$

hence using the absolute measure, $\gamma = r$.

4. The fundamental law of magneto-induction represents the electromotive force e as an expression of the mass of magnetic fluid μ, of the velocity of induced motion c, of the length of the induced element ds, and of its distance r from μ, and of a number given by the two angles ϕ, ψ which ds makes with r and c with the normal to the plane rds; that is, if the constant whose value is to be determined from the measures chosen is called δ,

$$e = \delta\frac{\mu c ds}{rr}\sin\phi\cos\psi.$$

The constant δ has the following signification:—If E is the absolute unit of measure of electromotive force, M the absolute unit of measure of magnetic fluid, S the seconds of time, and E′, M′, S′

the measures actually used, we get

$$\delta = \frac{EM'S}{E'MS'};$$

hence using the absolute measure, $\delta = 1$.

5. The fundamental law of voltaic induction represents the electromotive force e as an expression of the intensity i and of its change $\frac{di}{dt}$, of the velocity of the inducing motion c, and of the distance r of the induced from the inducing element, and of several numbers which are given by the relations of the lengths of the two elements to their distance $\frac{ds}{r}$, $\frac{ds'}{r}$, and by the four angles ϵ, θ, θ', ϕ which ds and c form with each other and with r, and which ds' forms with r; that is to say, if the constant whose value is to be determined from the measures chosen is called ζ,

$$e = \zeta\left[ci \cdot \frac{ds\,ds'}{rr}\left(\cos\epsilon - \frac{3}{2}\cos\theta\cos\theta'\right)\cos\phi + \frac{1}{2}\frac{di}{dt}\cdot\frac{ds\,ds'}{r}\cos\theta\cos\phi\right].$$

The constant ζ has the following significance:—If E and I are the absolute units for electromotive forces and for intensities, and C the absolute measure of velocity (a millimetre in a second), and E′, I′, C′ the measures actually used, we have

$$\zeta = 2\frac{EI'C'}{E'IC};$$

hence using the absolute measure itself,

$$\zeta = 2.$$

6. The general fundamental law of electric action represents the electric force F as an expression of the electric masses v, v', of their distance r, their relative velocity $\frac{ds}{dt}$, and their change $\frac{ddr}{dt^2}$; that is, if the constant whose value is to be determined from the given measures is called η, we have

$$F = \eta \cdot \frac{vv'}{rr}\left[1 - \frac{1}{aa}\left(\frac{dr^2}{dt^2} - 2r\frac{ddr}{dt^2}\right)\right];$$

a stands for the number indicating the relation of that velocity with which two electric masses must be moved against each other in order that they exert no force on each other, to the velocity of a millimetre in a second.

The constant η has the following signification:—If F is the absolute measure of force, N the absolute unit of electric fluid (that mass of electric fluid which at a distance of a millimetre

exerts upon a similar mass the absolute unit of force), if R is a millimetre, and F′, N′, R′ the measures actually used, we have

$$\eta = \frac{FN'N'RR}{F'NNR'R'};$$

hence using the absolute measure itself, $\eta = 1$

Every electric force can act, however, as electromotive force; and this latter e is represented, according to the general fundamental law of electric action, as an expression of the electric mass v, of the length of the element in which is contained the quantity of electricity acted upon; further, of the distance r of both from each other, of their relative velocity $\frac{dr}{dt}$, and their change $\frac{d\,dr}{dt^2}$, and of the angle ϕ which ds forms with r; that is, if the constant whose value is to be determined from the measures chosen is called k, we have

$$e = k \cdot \frac{vds}{rr}\left[a - 1\frac{1}{a}\left(\frac{dr^2}{dt^2} - 2r\frac{ddr}{dt}\right)\right]\cos\phi.$$

The constant k has the following meaning:—If E is the absolute unit of measure of electromotive forces, N the absolute unit of measure of the electric fluid, C the absolute unit of velocity (a millimetre in a second), R a millimetre, and E′, N′, C′, R′ the measures actually used, we have

$$k = \frac{1}{2\sqrt{2}}\frac{EN'C'R}{E'NCR'};$$

hence using the absolute measure,

$$k = \frac{1}{2\sqrt{2}}.$$

XXXV. *On the Action of Uncrystallized Films upon Common and Polarized Light.* *By* Sir David Brewster, *K.H., F.R.S.**

IN a paper "On the Polarization of Light by Refraction," published in the Philosophical Transactions for 1814, I have shown that when a pencil of light is incident on a number of uncrystallized plates, inclined at the same or different angles to the incident ray, all their surfaces being perpendicular to the plane of the first incidence, the transmitted pencil will be wholly polarized when the sum of the tangents of the angle of incidence upon each plate is equal to a constant quantity depending upon

* Communicated by the Author. This paper was read at the meeting of the British Association held at Aberdeen in Sept. 1859.

the refractive power of the plates and the intensity of the incident pencil.

This law, though admitted by M. Arago in his article on Polarization in the *Encyclopædia Britannica**, was called in question by Dr. Young †, on the ground that no finite number of plates could polarize the whole transmitted beam, as a small portion of light must always remain unpolarized, or in the state of natural light. This is doubtless true; but, as Sir John Herschel has shown, it does not affect the truth of the law, which involves the intensity of the incident pencil. According to the law of geometrical progression, indeed, a small portion of unpolarized light exists mathematically in the transmitted beam; but a beam of light may be said to be completely polarized when the unpolarized portion is invisible, vanishing entirely in certain positions of the analysing prism.

Neither M. Arago nor Dr. Young has made the slightest reference to that portion of the refracted light which is reflected at the surfaces of each plate and returned into the transmitted beam. Sir John Herschel, however, has distinctly referred to it, and remarks that "it mixes with the transmitted beam, and, being in an opposite plane, destroys a part of its polarization ‡."

Although the law of the tangents which I have mentioned refers only to the transmitted pencil, yet, in the paper which contains it, I have shown that the light reflected back into that pencil is distinctly visible, not as ordinary light, as Sir John Herschel maintains, but as light polarized in an opposite plane to the refracted pencil.

When the angle of incidence is considerable, this oppositely polarized light appears as a nebulous mass, like the nebulous image in the agate; and after examining it, I found it to have the same relation to the refracted pencil "*as the nebulous image has to the bright image of the agate, or as the first has to the second pencil of doubly refracting crystals* §."

In making the experiment with a small bright image of a candle, and using plates of parallel glass, I found that the reflected images *a*, *a*, *a*, *a* were distinctly separated from the bright or refracted image A, and were all polarized by reflexion in a plane opposite to that of A ‖.

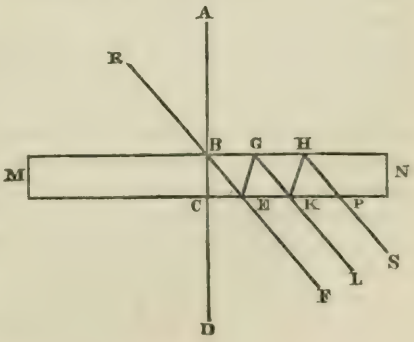
Although these two facts, which have much theoretical importance, were not only minutely described, but represented in diagrams, in my paper of 1814, yet they escaped the notice of the

* *Encyc. Brit.* vol. xviii. part 1. sect. v.
† Ibid., in the passage within brackets.
‡ Treatise on Light, art. 868.
§ Phil. Trans. 1814, p. 226, and plate 8. figs. 2, 3.
‖ Ibid.

three distinguished philosophers I have named, and of all subsequent writers; and the consequence of this has been that the true action of a pile of plates or films has never been the subject of research during the last forty-six years, though such piles have been used in some of the most delicate and important researches in physical optics.

The difficulty of procuring transparent plates with parallel surfaces, and of sufficient thinness, would have prevented the most skilful observer from making any progress in the inquiry; and had I not been fortunate enough to obtain, from the museum of the Marquis Campana in Rome, a large quantity of glass in different stages of decomposition, I could hardly have done more than confirm the result which I obtained in 1813, that the light transmitted by a pile of transparent plates consists of two portions of light polarized in opposite planes.

In submitting the films of decomposed glass to the polarizing microscope, I observed a number of polygonal portions, approaching more or less to circles, but often perfectly circular, and exhibiting the black cross with coloured sectors and rings analogous to those produced by uniaxal crystals. This observation, which was made with decomposed glass given me forty years ago by the late Marquis of Northampton, was communicated to the British Association at Glasgow in 1855; but at that time I regarded the black cross and its accompanying tints, as shown in the drawings on the table, as produced by the refraction and polarization in different azimuths of the light transmitted through the spherical shells, like a group of watch-glasses, of which the circular portions were composed. The light surrounding the black cross was so highly coloured with the colours of the thin plates which composed the film of glass, that I failed in every attempt to analyse it. After examining, however, many hundreds of these films from the new specimens which I have mentioned, I succeeded in finding a few in which there were no such colours, and which enabled me to arrive at results that could not have been obtained from the finest and the thinnest plates of glass artificially produced.



These results will be understood from the annexed diagram, in which M N is a thin plate and A B a ray of common light incident perpen-

dicularly at B, and emerging at C in the direction C D. As a portion of the ray B C is reflected at C, and again reflected at B and transmitted at C, the pencil C D will consist of two distinct portions, one of which has been twice refracted, and the other and much feebler portion twice reflected. As neither of these portions are polarized, no physical change is produced by their combination, unless when the plate M N is so extremely thin as to produce the colours of thin plates by the interference of the reflected with the refracted portions.

When a ray R B is incident obliquely at B, it suffers refraction at B and E, and the emergent pencil E F contains a portion of light polarized by refraction. This ray, in passing through other plates or films parallel to M N, is at last completely polarized in one plane, having grown feebler in intensity by the abstraction of the light reflected at the two surfaces of each plate.

The portion of the refracted pencil B E which is reflected at E and G, and a portion of it polarized, emerges at K as a pencil K L, partly polarized by reflexion. A portion of G K is again reflected at K and H, and emerges at P as a pencil P S, more polarized by reflexion than K L. Hence the principal or refracted pencil E F is combined with the pencils K L, P S (and others by reflexions at P, &c.), polarized in an opposite plane, so that with a certain number of plates, varying with the angle of incidence, the emergent pencil E F, K L, and P S consists of two oppositely-polarized portions of light approximately equal.

When polarized light is incident upon a pile of these thin and colourless films, and subsequently analysed, it exhibits all the properties of a plate cut perpendicularly to the axis of a uniaxal crystal. The line A D corresponds with the axis of the crystal; and the different azimuths in which the polarized ray may be inclined to this axis correspond with the principal sections of a uniaxal crystal. The polarized tints have the same value in every azimuth at the same angle of incidence, and therefore form rings which, when crossed with plates of sulphate of lime, descend in Newton's Scale like the tints of negative uniaxal crystals.

Out of hundreds of specimens now on the table, I have found a few so colourless and so perfect as to produce, at different incidences, all the polarized tints or rings up to the *blue* of the second order of Newton's Scale. These colours are so pure, and so regularly developed by the inclination of the plate, that the most skilful observer could not fail to pronounce it to be a portion of a doubly refracting crystal.

The production of the leading phenomena of doubly refracting crystals, namely, two oppositely polarized pencils, and the system

of coloured rings by the interference of these pencils, is certainly one of the most remarkable facts in physical optics; and, in a theoretical point of view, no less remarkable is the fact that one of the interfering portions is a fasciculus of pencils returned into the refracted beam by different routes, and having different origins.

Owing to the extreme thinness of the combined films, we cannot, as with thick plates of uniaxal crystals, see at once the black cross and its attendant rings; but in numerous specimens of decomposed glass to which I have already referred, the films are spherical shells of different diameters and thicknesses, and exhibit the black cross with the greatest sharpness and beauty. In many specimens these circular combinations are perfectly colourless, and the colours of the four luminous sectors which embrace the black cross rise only to the *white* of the first order.

When the films are so thin as to give the colours of thin plates, the colour of the luminous sectors is generally the same as that of the film in which the circular portions occur, and the rings or bands which surround them have a very peculiar character, owing to the manner in which the spherical shells are joined to the films which compose the plate.

How far these results may lead to new views of the structure which produces double refraction, it would be unprofitable to inquire in the present state of our knowledge of the atomical constitution of transparent bodies.

XXXVI. *On the Absorption and Radiation of Heat by Gases and Vapours, and on the Physical Connexion of Radiation, Absorption, and Conduction.—The Bakerian Lecture.* By John Tyndall, *Esq., F.R.S. &c.*

[Concluded from p. 194.]

§ 7. *Action of permanent Gases on Radiant Heat.*—The deportment of oxygen, nitrogen, hydrogen, atmospheric air, and olefiant gas has been already recorded. Besides these I have examined carbonic oxide, carbonic acid, sulphuretted hydrogen, and nitrous oxide. The action of these gases is so much feebler than that of any of the vapours referred to in the last section, that, in examining the relationship between absorption and density, the measures used with the vapours were abandoned, and the quantities of gas admitted were measured by the depression of the mercurial gauge.

Table XIX.—Carbonic Oxide.

Tension in inches.	Absorption. Observed.	Calculated.
0·5	2·5	2·5
1·0	5·6	5·0
1·5	8·0	7·5
2·0	10·0	10·0
2·5	12·0	12·5
3·0	15·0	15·0
3·5	17·5	17·5

Up to a tension of $3\frac{1}{2}$ inches the absorption by carbonic oxide is proportional to the density of the gas. But this proportion does not obtain with large quantities of the gas, as shown by the following Table:—

Tension in inches.	Deflection.	Absorption.
5	18°·0	18
10	32·5	32·5
15	41·0	45

Table XX.—Carbonic Acid.

Tension in inches.	Absorption. Observed.	Calculated.
0·5	5·0	3·5
1·0	7·5	7·0
1·5	10·5	10·5
2·0	14·0	14·0
2·5	17·8	17·5
3·0	21·8	21·0
3·5	24·5	24·5

Here we have the proportion exhibited, but not so with larger quantities.

Tension in inches.	Deflection.	Absorption.
5	25°·0	25
10	36·0	36
15	42·5	48

Table XXI.—Sulphuretted Hydrogen.

Tension in inches.	Absorption. Observed.	Calculated.
0·5	7·8	6
1·0	12·5	12
1·5	18·0	18
2·0	24·0	24
2·5	30·0	30
3·0	34·5	36
3·5	36·0	42
4·0	36·5	48
4·5	38·0	54
5·0	40·0	60

The proportion here holds good up to a tension of 2·5 inches, when the deviation from it commences and gradually augments.

Though these measurements were made with all possible care, I should like to repeat them. Dense fumes issued from the cylinders of the air-pump on exhausting the tube of this gas, and I am not at present able to state with confidence that a trace of such in a very diffuse form within the tube did not interfere with the purity of the results.

TABLE XXII.—Nitrous Oxide.

	Absorption.	
Tension in inches.	Observed.	Calculated.
0·5	14·5	14·5
1·0	23·5	29·0
1·5	30·0	43·5
2·0	35·5	58·0
2·5	41·0	71·5
3·0	45·0	87·0
3·5	47·7	101·5
4·0	49·0	116·0
4·5	51·5	130·5
5·0	54·0	145·0

Here the divergence from proportionality makes itself manifest from the commencement.

I promised at the first page of this memoir to allude to the results of Dr. Franz, and I will now do so. With a tube 3 feet long and blackened within, an absorption of 3·54 per cent. by atmospheric air was observed in his experiments. In my experiments, however, with a tube 4 feet long and polished within, which makes the distance traversed by the reflected rays more than 4 feet, the absorption is only one-tenth of the above amount. In the experiments of Dr. Franz, carbonic acid appears as a feebler absorber than oxygen. According to my experiments, for small quantities the absorptive power of the former is about 150 times that of the latter; and for atmospheric tensions, carbonic acid probably absorbs nearly 100 times as much as oxygen.

The differences between Dr. Franz and myself admit, perhaps, of the following explanation. His source of heat was an argand lamp, and the ends of his experimental tube were stopped with plates of glass. Now Melloni has shown that fully 61 per cent. of the heat-rays emanating from a Locatelli lamp are absorbed by a plate of glass one-tenth of an inch in thickness. Hence in all probability the greater portion of the rays issuing from the lamp of Dr. Franz was expended in heating the two glass ends of his experimental tube. These ends thus became secondary sources of heat which radiated against his pile. On admitting air into the tube, the partial withdrawal by conduction and con-

vection of the heat of the glass plates would produce an effect exactly the same as that of true absorption. By allowing the air in my tube to come into contact with the radiating plate, I have often obtained a deflection of twenty or thirty degrees,—the effect being due to the cooling of the plate, and not to absorption. It is also certain that had I used heat from a luminous source, I should have found the absorption of 0·33 per cent. considerably diminished.

§ 8. I have now to refer briefly to a point of considerable interest as regards the effect of our atmosphere on solar and terrestrial heat. In examining the separate effects of the air, carbonic acid, and aqueous vapour of the atmosphere, on the 20th of last November, the following results were obtained:—

Air sent through the system of drying-tubes and through the caustic-potash tube produced an absorption of about . . 1.

Air direct from the laboratory, containing therefore its carbonic acid* and aqueous vapour, produced an absorption of . 15.

Deducting the effect of the gaseous acids, it was found that the quantity of aqueous vapour diffused through the atmosphere on the day in question, produced an absorption at least equal to thirteen times that of the atmosphere itself.

It is my intention to repeat and extend these experiments on a future occasion†; but even at present conclusions of great importance may be drawn from them. It is exceedingly probable that the absorption of the solar rays by the atmosphere, as established by M. Pouillet, is mainly due to the watery vapour contained in the air. The vast difference between the temperature of the sun at midday and in the evening, is also probably due in the main to that comparatively shallow stratum of aqueous vapour which lies close to the earth. At noon the depth of it pierced by the sunbeams is very small; in the evening very great in comparison.

The intense heat of the sun's direct rays on high mountains is not, I believe, due to his beams having to penetrate only a small depth of air, but to the comparative absence of aqueous vapour at those great elevations.

But this aqueous vapour, which exercises such a destructive action on the obscure rays, is comparatively transparent to the rays of light. Hence the differential action, as regards the heat coming from the sun to the earth and that radiated from the earth into space, is vastly augmented by the aqueous vapour of the atmosphere.

* And a portion of sulphurous acid produced by the two gas-lamps used to heat the cubes.

† The peculiarities of the locality in which this experiment was made render its repetition under other circumstances necessary.

De Saussure, Fourier, M. Pouillet, and Mr. Hopkins regard this interception of the terrestrial rays as exercising the most important influence on climate. Now if, as the above experiments indicate, the chief influence be exercised by the aqueous vapour, every variation of this constituent must produce a change of climate. Similar remarks would apply to the carbonic acid diffused through the air, while an almost inappreciable admixture of any of the hydrocarbon vapours would produce great effects on the terrestrial rays and produce corresponding changes of climate. It is not, therefore, necessary to assume alterations in the density and height of the atmosphere to account for different amounts of heat being preserved to the earth at different times; a slight change in its variable constituents would suffice for this. Such changes in fact may have produced all the mutations of climate which the researches of geologists reveal. However this may be, the facts above cited remain; they constitute true causes, the *extent* alone of the operation remaining doubtful.

The measurements recorded in the foregoing pages constitute only a fraction of those actually made; but they fulfil the object of the present portion of the inquiry. They establish the existence of enormous differences among colourless gases and vapours as to their action upon radiant heat; and they also show that, when the quantities are sufficiently small, the absorption in the case of each particular vapour is exactly proportional to the density.

These experiments furnish us with purer cases of molecular action than have been hitherto attained in experiments of this nature. In both solids and liquids the cohesion of the particles is implicated; they mutually control and limit each other. A certain action, over and above that which belongs to them separately, comes into play and embarrasses our conceptions. But in the cases above recorded the molecules are perfectly free, and we fix upon them individually the effects which the experiments exhibit; thus the mind's eye is directed more firmly than ever on those distinctive physical qualities whereby a ray of heat is stopped by one molecule and unimpeded by another.

§ 9. *Radiation of Heat by Gases.*—It is known that the quantity of light emitted by a flame depends chiefly on the incandescence of solid matter,—the brightness of an ignited jet of ordinary gas, for example, being chiefly due to the solid particles of carbon liberated in the flame.

Melloni drew a parallel between this action and that of radiant heat. He found the radiation from his alcohol lamp greatly augmented by plunging a spiral of platinum wire into the flame. He also found that a bundle of wire placed in the current of hot air ascending from an argand chimney gave a copious radia-

tion, while when the wire was withdrawn no trace of radiant heat could be detected by his apparatus. He concluded from this experiment that air possesses the power of radiation in so feeble a degree, that our best thermoscopic instruments fail to detect this power*.

These are the only experiments hitherto published upon this subject; and I have now to record those which have been made in connexion with the present inquiry. The pile furnished with its conical reflector was placed upon a stand, with a screen of polished tin in front of it. An alcohol lamp was placed behind the screen so that its flame was entirely hidden by the latter; on rising above the screen, the gaseous column radiated its heat against the pile and produced a considerable deflection. The same effect was produced when a candle or an ordinary jet of gas was substituted for the alcohol lamp.

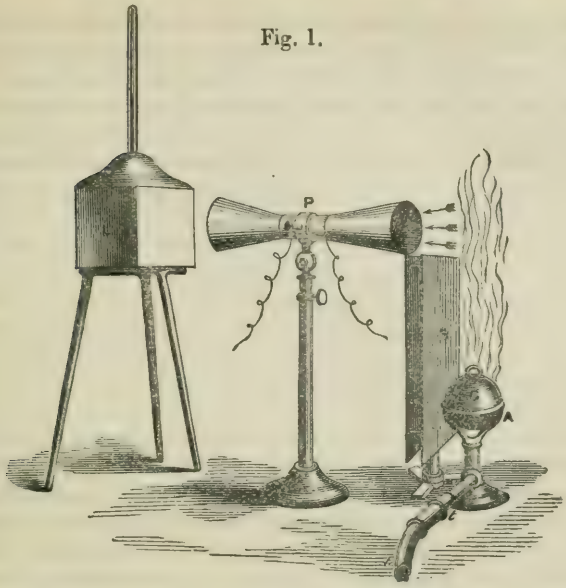
The heated products of combustion acted on the pile in the above experiments, but the radiation from pure air was easily demonstrated by placing a heated iron spatula or metal sphere behind the screen. A deflection was thus obtained which, when the spatula was raised to a red heat, amounted to more than sixty degrees. This action was due solely to the radiation of the air; no radiation from the spatula to the pile was possible, and no portion of the heated air itself approached the pile so as to communicate its warmth by contact to the latter. These effects are so easily produced that I am at a loss to account for the inability of so excellent an experimenter as Melloni to obtain them.

My next care was to examine whether different gases possessed different powers of radiation; and for this purpose the following arrangement was devised. P (fig. 1) represents the thermo-electric pile with its two conical reflectors; S is a double screen of polished tin; A is an argand burner consisting of two concentric rings perforated with orifices for the escape of the gas; C is a heated copper ball; the tube *t t* leads to a gas-holder containing the gas to be examined. When the ball C is placed on the argand burner, it of course heats the air in contact with it; an ascending current is established, which acts on the pile as in the experiments last described. It was found necessary to neutralize this radiation from the heated air, and for this purpose a large Leslie's cube L, filled with water a few degrees above the temperature of the air, was allowed to act on the opposite face of the pile.

When the needle was thus brought to zero, the cock of the gas-holder was turned on; the gas passed through the burner, came into contact with the ball, and ascended afterwards in a heated column in front of the pile. The galvanometer was now

* La Thermochrose, p. 94.

Fig. 1.



observed, and the limit of the arc through which its needle was urged was noted. It is needless to remark that the ball was entirely hidden by the screen from the thermo-electric pile, and that, even were this not the case, the mode of neutralization adopted would still give us the pure action of the gas.

The results of the experiments are given in the following Table, the figure appended to the name of each gas marking the number of degrees through which the radiation from the latter urged the needle of the galvanometer*:—

	C
Air	0
Oxygen	0
Nitrogen	0
Hydrogen	0
Carbonic oxide . .	12
Carbonic acid . . .	18
Nitrous oxide . . .	29
Olefiant gas . . .	53

The radiation from air, it will be remembered, was neutralized by the large Leslie's cube, and hence the 0° attached to it merely denotes that the propulsion of air from the gas-holder through the argand burner did not augment the effect. Oxygen, hydro-

* I have also rendered these experiments on radiation visible to a large audience. They may be readily introduced in lectures on radiant heat.

gen, and nitrogen, sent in a similar manner over the ball, were equally ineffective. The other gases, however, not only exhibit a marked action, but also marked differences of action. Their radiative powers follow precisely the same order as their powers of absorption. In fact, the deflections actually produced by their respective absorptions at 5 inches tension are as follow :—

Air	A fraction of a degree
Oxygen . . .	„ „
Nitrogen . .	„ „
Hydrogen . .	„ „
Carbonic oxide . . .	18°
Carbonic acid	25°
Nitrous oxide	44°
Olefiant gas	61°

It would be easy to give these experiments a more elegant form, and to arrive at greater accuracy, which I intend to do on a future occasion; but my object now is simply to establish the general order of their radiative powers. An interesting way of exhibiting both radiation and absorption is as follows:—When the polished face of a Leslie's cube is turned towards a thermo-electric pile the effect produced is inconsiderable, but it is greatly augmented when a coat of varnish is laid upon the polished surface. Instead of the coat of varnish, a film of gas may be made use of. Such a cube, containing boiling water, had its polished face turned towards the pile, and its effect on the galvanometer neutralized in the usual manner. The needle being at 0°, a film of olefiant gas, issuing from a narrow slit, was passed over the metal. The increase of radiation produced a deflection of 45°. When the gas was cut off, the needle returned accurately to 0°.

The absorption by a film may be shown by filling the cube with cold water, but not so cold as to produce the precipitation of the aqueous vapour of the atmosphere. A gilt copper ball, cooled in a freezing mixture, was placed in front of the pile, and its effect was neutralized by presenting a beaker containing a little iced water to the opposite face of the pile. A film of olefiant gas was sent over the ball, but the consequent deflection proved that the absorption, instead of being greater, was less than before. The ball, in fact, had been coated by a crust of ice, which is one of the best absorbers of radiant heat. The olefiant gas, being warmer than the ice, partially neutralized its absorption. When, however, the temperature of the ball was only a few degrees lower than that of the atmosphere, and its surface quite dry, the film of gas was found to act as a film of varnish; it augmented the absorption.

A remarkable effect, which contributed at first to the complexity of the experiments, can now be explained. Conceive the experimental tube exhausted and the needle at zero; conceive a small quantity of alcohol or ether vapour admitted; it cuts off a portion of the heat from one source, and the opposite source triumphs. Let the consequent deflection be 45°. If dry air be now admitted till the tube is filled, its effect of course will be slightly to augment the absorption and make the above deflection greater. But the following action is really observed:—when the air first enters, the needle, instead of ascending, descends; it falls to 26°, as if a portion of the heat originally cut off had been restored. At 26°, however, the needle stops, turns, moves quickly upwards, and takes up a permanent position a little higher than 45°. Let the tube now be exhausted, the withdrawal of the mixed air and vapour ought of course to restore the equilibrium with which we started; but the following effects are observed:—When the exhaustion commences, the needle moves upwards from 45° to 54°; it then halts, turns, and descends speedily to 0°, where it permanently remains.

After many trials to account for the anomaly, I proceeded thus:—A thermo-electric couple was soldered to the external surface of the experimental tube, and its ends connected with a galvanometer. When air was admitted, a deflection was produced, which showed that the air, on entering the vacuum, was heated. On exhausting, the needle was also deflected, showing that the interior of the tube was chilled. These are indeed known effects; but I was desirous to make myself perfectly sure of them. I subsequently had the tube perforated and thermometers screwed into it air-tight. On filling the tube the thermometric columns rose, on exhausting it they sank, the range between the maximum and minimum amounting in the case of air to 5° Fahr.

Hence the following explanation of the above singular effects. The absorptive power of the vapour referred to is very great, and its radiative power is equally so. The heat generated by the air on its entrance is communicated to the vapour, which thus becomes a temporary source of radiant heat, and diminishes the deflection produced in the first instance by its presence. The reverse occurs when the tube is exhausted; the vapour is chilled, its great absorptive action on the heat radiated from the adjacent face of the pile comes more into play, and the original effect is augmented. In both cases, however, the action is transient; the vapour soon loses the heat communicated to it, and soon gains the heat which it has lost, and matters then take their normal course.

§ 10. *On the Physical Connexion of Radiation, Absorption, and*

Conduction.—Notwithstanding the great accessions of late years to our knowledge of the nature of heat, we are as yet, I believe, quite ignorant of the atomic conditions on which radiation, absorption, and conduction depend. What are the specific qualities which cause one body to radiate copiously and another feebly? Why, on theoretic grounds, must the equivalence of radiation and absorption exist? Why should a highly diathermanous body, as shown by Mr. Balfour Stewart, be a bad radiator, and an adiathermanous body a good radiator? How is heat conducted? and what is the strict physical meaning of good conduction and bad conduction? Why should good conductors be, in general, bad radiators, and bad conductors good radiators? These, and other questions, referring to facts more or less established, have still to receive their complete answers. It is less with a hope of furnishing such than of shadowing forth the possibility of uniting these various effects by a common bond, that I submit the following reflections to the notice of the Royal Society.

In the experiments recorded in the foregoing pages, we have dealt with *free* atoms, both simple and compound, and it has been found that in all cases absorption takes place. The meaning of this, according to the dynamical theory of heat, is that no atom is capable of existing in vibrating ether without accepting a portion of its motion. We may, if we wish, imagine a certain roughness of the surface of the atoms which enables the ether to *bite* them and carry the atom along with it. But no matter what the quality may be which enables any atom to accept motion from the agitated ether, the same quality must enable it to impart motion to still ether when it is plunged in the latter and agitated. It is only necessary to imagine the case of a body immersed in water to see that this must be the case. There is a polarity here as rigid as that of magnetism. From the existence of absorption, we may on theoretic grounds infallibly infer a capacity for radiation; from the existence of radiation, we may with equal certainty infer a capacity for absorption; and each of them must be regarded as the measure of the other*.

This reasoning, founded simply on the mechanical relations of the ether and the atoms immersed in it, is completely verified by experiment. Great differences have been shown to exist among gases as to their powers of absorption, and precisely similar differences as regards their powers of radiation. But what specific property is it which makes one free molecule a strong absorber, while another offers scarcely any impediment to the passage of radiant heat? I think the experiments throw

* This was written long before Kirchhoff's admirable papers on the relation of emission to absorption were known to me.

some light upon this question. If we inspect the results above recorded, we shall find that the *elementary* gases hydrogen, oxygen, nitrogen, and the *mixture* atmospheric air, possess absorptive and radiative powers beyond comparison less than those of the *compound* gases. Uniting the atomic theory with the conception of an ether, this result appears to be exactly what ought to be expected. Taking Dalton's idea of an elementary body as a single sphere, and supposing such a sphere to be set in motion in still ether, or placed without motion in moving ether, the communication of motion by the atom in the first instance, and the acceptance of it in the second, must be less than when a number of such atoms are grouped together and move as a system. Thus we see that hydrogen and nitrogen, which, when *mixed* together, produce a small effect, when *chemically united* to form ammonia, produce an enormous effect. Thus oxygen and hydrogen, which, when mixed in their electrolytic proportions, show a scarcely sensible action, when chemically combined to form aqueous vapour exert a powerful action. So also with oxygen and nitrogen, which, when mixed, as in our atmosphere, both absorb and radiate feebly, when united to form oscillating systems, as in nitrous oxide, have their powers vastly augmented. Pure atmospheric air, of 5 inches tension, does not effect an absorption equivalent to more than the one-fifth of a degree, while nitrous oxide of the same tension effects an absorption equivalent to fifty-one such degrees. Hence the absorption by nitrous oxide at this tension is about 250 times that of air. No fact in chemistry carries the same conviction to my mind, that air is a *mixture* and not a *compound*, as that just cited. In like manner, the absorption by carbonic oxide of this tension is nearly 100 times that of oxygen alone; the absorption by carbonic acid is about 150 times that of oxygen; while the absorption by olefiant gas of this tension is 1000 times that of its constituent hydrogen. Even the enormous action last mentioned is surpassed by the vapours of many of the volatile liquids, in which the atomic groups are known to attain their highest degree of complexity.

I have hitherto limited myself to the consideration, that the compound molecules present broad sides to the ether, while the simple atoms with which we have operated do not,—that in consequence of these differences the ether must swell into billows when the former are moved, while it merely trembles into ripples when the latter are agitated,—that, in the interception of motion also, the former, other things being equal, must be far more influential than the latter; but another important consideration remains. All the gases and vapours whose deportment we have examined are transparent to light; that is to say,

the waves of the visible spectrum pass among them without sensible absorption. Hence it is plain that their absorptive power depends on the periodicity of the undulations which strike them. At this point the present inquiry connects itself with the experiments of Niépce, the observation of Foucault, the surmises of Ängstrom, Stokes, and Thomson, and those splendid researches of Kirchhoff and Bunsen which so immeasurably extend our experimental range. By Kirchhoff it has been conclusively shown that every atom absorbs in a special degree those waves which are synchronous with its own periods of vibration. Now, besides presenting broader sides to the ether, the association of simple atoms to form groups must, as a general rule, render their motions through the ether more sluggish, and tend to bring the periods of oscillation into isochronism with the slow undulations of obscure heat, thus enabling the molecules to absorb more effectually such rays as have been made use of in our experiments.

Let me here state briefly the grounds which induce me to conclude that an agreement in period alone is not sufficient to cause powerful absorption and radiation—that in addition to this the molecules must be so constituted as to furnish *points d'appui* to the ether. The heat of contact is accepted with extreme freedom by rock-salt, but a plate of the substance once heated requires a great length of time to cool. This surprised me when I first noticed it. But the effect is explained by the experiments of Mr. Balfour Stewart, by which it is proved that the radiative power of heated rock-salt is extremely feeble. Periodicity can have no influence here, for the ether is capable of accepting and transmitting impulses of all periods; and the fact that rock-salt requires more time to cool than alum, simply proves that the molecules of the former glide through the ether with comparatively small resistance, and thus continue moving for a longer time; while those of the latter presenting broad sides to the ether, speedily communicate to it the motion which we call heat. This power of gliding through still ether possessed by the rock-salt molecules, must of course enable the moving ether to glide round them, and no coincidence of period could, I think, make such a body a powerful absorber.

Many chemists, I believe, are disposed to reject the idea of an atom, and to adhere to that of equivalent proportions merely. They figure the act of combination as a kind of interpenetration of one substance by another. But this is a mere masking of the fundamental phenomenon. The value of the atomic theory consists in its furnishing the physical explanation of the law of equivalents: assuming the one, the other follows; and assuming the act of chemical union as Dalton figured it, we see that it

blends harmoniously with the perfectly independent conception of an ether, and enables us to reduce the phenomena of radiation and absorption to the simplest mechanical principles.

Considerations similar to the above may, I think, be applied to the phenomena of *conduction*. In the Philosophical Magazine for August 1853, I have described an instrument used in examining the transmission of heat through cubes of wood and other substances. When engaged with this instrument, I had also cubes of various crystals prepared, and determined with it their powers of conduction. With one exception, I found that the conductivity augmented with the diathermancy. The exception was furnished by a cube of very perfect rock-crystal, which conducted slightly better than my cube of rock-salt. The latter, however, had a very high conductive power; in fact rock-salt, calcareous spar, glass, selenite, and alum stood in my experiments, as regards conductivity, exactly in their order of diathermancy in the experiments of Melloni. I have already adduced considerations which show that the molecules of rock-salt glide with facility through the ether; but the case of motion which these molecules enjoy must facilitate their mutual collision. Their motion, instead of being expended on the ether which exists between them, and communicated by it to the external ether, is in great part transferred directly from particle to particle, or in other words, is freely conducted. When a molecule of alum, on the contrary, approaches a neighbour molecule, it produces a swell in the intervening ether, which swell is in part transmitted, not to the molecules, but to the general ether of space, and thus lost as regards conduction. This lateral waste prevents the motion from penetrating the alum to any great extent, and the substance is what we call a bad conductor*.

Such considerations as these could hardly occur without carrying the mind to the kindred question of electric conduction; but the speculations have been pursued sufficiently far for the present, and must now abide the judgment of those competent to decide whether they are the mere emanations of fancy, or a fair application of principles which are acknowledged to be secure.

The present paper, I may remark, embraces only the first section of these researches.

* In the above considerations regarding conduction, I have limited myself to the illustration furnished by two compound bodies; but the elementary atoms also differ among themselves as regards their powers of accepting motion from the ether and of communicating motion to it. I should infer, for example, that the atoms of platinum encounter more resistance in moving through the ether than the atoms of silver. It is needless to say that the physical texture of a substance also has a great influence.

XXXVII. *Experimental and Theoretical Researches on the Figures of Equilibrium of a Liquid Mass devoid of Weight.*—Fifth Series *. *By* M. J. Plateau †.

New process for the production of figures in a state of equilibrium.—Pressure exerted by a liquid spherical film on the air which it contains.—Investigation of the very small limit within which, in a particular liquid, the value of radius of appreciable molecular attraction varies.

IN the Second and Fourth Series of this investigation I have applied my process of the immersion of a mass of oil in a mixture of water and alcohol to the production of some of the figures in a state of equilibrium which pertain to a liquid mass, supposed to be devoid of gravity and in a state of repose. This process, so simple in principle, presents in practice certain difficulties, and it required a certain cleverness to arrive at perfectly regular results. In the present series, I shall point out a process wholly different, far more simple and more convenient, and entirely exempt from the inconveniences of the previously described plan; I shall demonstrate afterwards some of the numerous results which the employment of the new method has furnished me, and the theoretical principles on which it rests.

I may remark in the first place, that oil immersed in the alcoholic mixture is easily converted into thin films; I shall show, for example, that, with a number of precautions which I describe, one can obtain, in the mixture in question, a hollow bubble of oil more than 12 centimetres in diameter, by inflating it with the same alcoholic mixture, just as one obtains in air a soap-bubble filled with air itself.

It must be remembered, with regard to these films of oil, that in the experiment in my First Series where a ring of oil is formed, this ring remains at first united to the central apparatus by a thin film; and starting with that fact, I shall show once more the incorrectness of every deduction, derived from this experiment, in favour of a cosmogonic hypothesis.

After having thus established the facilities for the production of liquid films removed from the action of gravity, I shall demonstrate that the figures in a state of equilibrium which appertain to the liquid films devoid of weight, are identically the same as those of full liquid masses, likewise deprived of weight.

* For the preceding Series see Taylor's Scientific Memoirs, Parts XIII. and XXI; and Phil. Mag. (S. 4), vol. xiv. p. 1, and vol. xvi. p. 23.

† The original memoir will be found in the thirty-third volume of the *Mémoires de l'Académie de Bruxelles*. The abstract, of which a translation is here given, appeared in the *Annales de Chimie et de Physique* for June 1861.

Besides, it is possible, without having recourse to mathematical analysis, sufficiently to account for this identity. Let me repeat, for this purpose, a principle on which I have many times dwelt in the preceding series. When a surface fulfils the general condition of equilibrium, it is indifferent whether the liquid be on one side or on the other of this surface; in other words, to each figure in a state of equilibrium, which is in relief, corresponds a figure in a state of equilibrium, identical with the same, only in depression. Now, the two faces of a liquid film, on account of the thinness of the latter, being capable of being considered as though they were two identical surfaces, the one in relief and the other depressed with regard to the liquid which forms the film, it follows, from the principle in question, that if one of these two faces constitutes a surface of equilibrium, it is the same with the other face, and that thus equilibrium exists throughout the entire film.

Suppose, however, it was possible to form in air liquid films devoid of weight; these films would necessarily take the same form as the films of oil formed in the alcoholic mixture. Now liquid films formed in air (films of soapy water, for example) are so thin that the action of gravity upon them can generally be regarded as inappreciable in comparison with that of molecular forces; we should therefore obtain in air, with films of soapy water or of an analogous liquid, the same figures in a state of equilibrium as with films of oil in the alcoholic mixture, and consequently, after what I have said above, figures which would belong to a full liquid mass devoid of weight. Therein consists the process which I have mentioned.

Thus we arrive at the curious result, that, with a liquid acted upon by gravity and in a state of repose, one can produce on a large scale all forms of equilibrium which belong to a liquid mass without weight and likewise at rest.

Soap-bubbles offer the first example for the employment of the process under consideration; floating in air, they are spherical, just as a full liquid mass would be if devoid of weight and freed from all adhesion.

The films, however, which are obtained from common solution of soap have but a very short existence, unless they be in a close vessel; a soap-bubble of one decimetre diameter, formed in the open air of a room, rarely lasts two minutes; it was therefore essential to find out some better liquid; and I have been happy enough to discover one which furnishes in the open air, whilst preserving its liquid nature, films of great durability. This liquid is formed by mixing, in proper proportions, glycerine, water, and soap. A glycerine which seems very pure and very concentrated can be easily procured at no great cost in London, at Mr. Bolton's, 146 Holborn Bars, for instance. I shall point

out in a note at the end of the memoir, the proper way to obtain sufficiently good results with the ordinary glycerine of commerce.

The mixture must be prepared in summer, and when the temperature out-of-doors is at least 19° Cent. Dissolve at a gentle heat one part by weight of Marseilles soap, previously cut into thin shavings, in 40 parts of distilled water; and when the solution is cold, filter it. That done, carefully mix in a flask by violent and continual agitation, 2 vols. of glycerine with 3 vols. of the above-mentioned solution, and then allow it to stand. The mixture, limpid at the time of its formation, begins after some hours to grow turbid; a slight white precipitate is produced, which rises with great slowness, and after some days forms a distinct layer at the top of the liquid; the limpid portion is then collected by means of a siphon, which draws off by a lateral tube, and the preparation is at an end.

The liquid thus obtained, and which I name *glyceric liquid*, gives films of great durability; for instance, if with this liquid, by means of a common clay pipe, a bubble 1 decimetre in diameter be inflated, and then placed in the open air of a room upon a ring of iron wire 4 centimetres in diameter and previously moistened with the same liquid, this bubble, provided it is perfectly at rest, will remain entire for three hours.

The glyceric liquid can be preserved about a year, after which time it rapidly decomposes. I have not observed any disengagement of gas; however, as the liquid is of an organic nature, it would not be unlikely that such might be produced sometimes; and it would be prudent, to prevent a possible explosion, to only close the flask with a cork which does not fit very firmly.

Just as the films of soapy water last very much longer in a closed vessel than in the open air, the endurance of the films of glyceric liquid, already so great in the open air, becomes still much more considerable when these films are enclosed in a vessel, especially if certain precautions are taken. I shall quote an example of this further on.

Having thus obtained a liquid easily furnishing films both large and very durable, I employ it in order to produce by means of them all the figures, in a state of equilibrium, of revolution. In order not to give this analysis too great length, I shall limit myself here to succinctly describing the formation of the cylinder.

For this purpose, use is made of an apparatus of two rings of iron wire 7 centimetres in diameter, similar to those mentioned by me in the preceding series, that is to say, the lower ring on a tripod, and the upper ring, supported by a fork, fixed into the two extremities of its diameter; the end of this fork is attached to a support, fixed in such a manner that the ring can

be raised or lowered by a gentle movement. Place the first ring upon its stand on the table, sustain the second at a convenient height over it, and well moisten both with the glyceric liquid; then inflate a bubble of about 10 centimetres diameter, place it upon the lower ring and withdraw the pipe; now lower the upper ring until it comes in contact with the bubble, which immediately attaches itself to it; at last gradually raise this ring, and the bubble, which thus drawn out loses more and more its spherical curvature, is converted, by a certain separation of the rings, into a perfectly regular cylinder, having convex bases like the full cylinders of oil.

A rather larger diameter can be given to the bubble; but when it is too large, the cylindrical form is no longer obtained, either because the cylinder which it is desired to obtain exceeds its limit of stability*, or because, if it be still within this limit, it begins to approach it; in this last case, in fact, the figure-producing forces becoming very little intense, the small weight of the film exerts an appreciable influence, and the figure appears more or less swollen at the lower half, and compressed at the upper half. The tallest regular cylinder which can be formed with the rings, before pointed out, has a height of about 17 centimetres. Let us state in this place, that, for the complete success of experiments of this kind, the rings should have undergone a little preparation: when they leave the hands of the workman they should be slightly oxidized on their surface by dipping them for two minutes into nitric acid diluted with four times its volume of water; afterwards wash them in pure water.

In the memoir will be found the way to produce, in the laminated condition as well, the other forms of equilibrium of revolution, namely, those to which I have given† the names of *catenoids*, *onduloids*, and *nodoids*.

These experiments are very curious; there is a peculiar charm in the contemplation of these figures, so slender, almost reduced to mathematical surfaces, which make their appearance tinted with the most brilliant colours, and which, in spite of their extreme frailness, endure for such a time. These same experiments can be readily performed, and in the most convenient manner.

I now pass on to another application of my new process. Procure a collection of frames of iron wire, each one of which exhibits all the edges of a polyhedron—for example, of a cube, a regular octahedron, of prisms with triangular, pentagonal, and other bases. Each of these frames is to be fixed like the upper ring in the before-mentioned experiment, by a fork attached to two of its edges; they ought also to be oxidized by nitric acid.

* See Second Series in Scientific Memoirs, Part XXI.

† See the abstract of the Fourth Series in Phil. Mag. vol. xvi. p. 23.

In order to give an idea of the most convenient dimensions for this apparatus, I will just say that the edges of my cubical frame are 7 centimetres in length, and that the iron wire, of which it is formed, is a little less than 1 millimetre in thickness. I have already employed similar frames in the experiments (mentioned in my second memoir) for the formation of liquid polyhedra.

If one of these frames were completely dipped (with the exception of the upper part of the fork) into the glyceric liquid and then withdrawn, it would be expected that the adhesion of this liquid to the solid frame would cause the formation of a set of films, occupying the interior of the frame; and this *does* take place; but a most remarkable thing it is, that the arrangement of these films is not a matter of chance; it is, on the contrary, perfectly regular and perfectly constant for each frame. In the cubical frame, for instance, is invariably obtained a collection of twelve films, starting respectively from the twelve wires, and all converging on a much smaller thirteenth one of quadrangular form and occupying the centre of the apparatus.

These systems of films, thus prepared in these polyhedral frames, have excited the admiration of all to whom I have shown them; they have a perfect regularity; the liquid edges that join among them the films of which they are composed are extremely fine, and the films themselves after some time exhibit the richest colours; again, the arrangement of these same films is regulated by simple and uniform laws, which I shall examine from a theoretical point of view in the next series, and of which here are the two principles:—

I. At one and the same liquid edge never more than three films can meet, and these same are inclined to each other at equal angles.

II. When several liquid edges meet at one and the same point in the interior of a system, these edges are always four in number, and are inclined to each other, at the point in question, at equal angles.

I had already obtained, by totally different means, these systems of films with oil immersed in the alcoholic liquid, as will be seen in my second series; but they are far less perfect and far less easily produced than by my present process.

We now pass on to another subject. It is well known that a soap-bubble exerts a pressure on the air which it contains. Mr. Henry, in an oral communication made in 1844 to the American Society, has described experiments by means of which he measured this pressure by the height of the column of water with which it is in equilibrium; but I believe that his numbers have not been published. I have looked at the question in a general way from a theoretical point of view, and have arrived at the following result:—Let ρ stand for the density of

the liquid of which the film is formed, h the height to which the same liquid rises in a capillary tube of 1 millimetre internal diameter, d the diameter of the bubble, and lastly, let p denote the pressure which this bubble exerts, or, more precisely speaking, the height of the column with which it would be in equilibrium; then this pressure is expressed by the formula

$$p=\frac{2h\rho}{d}.$$

The product $h\rho$ is, as can easily be shown, proportional to the cohesion of the liquid; the pressure exerted by a bubble upon its enclosed air is consequently in direct ratio to the cohesion of the liquid, and in inverse ratio to the diameter of the bubble.

I verify my formula by the experiment with the glyceric liquid. By means of my apparatus, which is merely Mr. Henry's slightly modified, a bubble is inflated at the orifice of a small inverted funnel which communicates with a water-manometer. The difference of the level in the two branches of this instrument is measured by means of a cathetometer; and the latter is likewise employed to measure the diameter of the bubble, for which purpose it is placed in a horizontal position on suitable supports.

The formula gives $hd=2h\rho$,

which shows that the product of the pressure by the diameter must be constant for the same liquid and at the same temperature, since under these conditions h and ρ do not vary: it is this constant which I first of all sought to verify. These measurements have been made by means of ten bubbles, of which the smallest had a diameter of 7·55 millimetres, and the longest a diameter of 48·1 millimetres, and consequently within limits which were to one another nearly as 1 to 6; the temperature ranged between 18°·5 and 20°.

The mean of the ten values obtained for the product pd is 22·75. Except in the case of the two largest diameters, there was very little difference from the general mean; and if the results are arranged in order, with the diameters increasing, it will be perceived that these small discrepancies are irregularly distributed. The two values which form exceptions are 20·57 and 26·45, and it is seen that the first is under the mean, whilst the second is over it. As the other eight values presented a remarkable agreement, I have deemed it allowable to reject, as spoiled by errors of accident, the two that I have mentioned, and I thought I might take, in order to estimate the product pd as regards the glyceric liquid, the mean of the eight agreeing determinations, which mean is 22·56.

It remains to compare the value of the product pd, thus deduced by experiment, with that which our formula gives; and

for this purpose it was necessary to determine, at the temperature of the preceding experiment, the density ρ and the height h in regard to the glyceric liquid. This I have done, employing every known precaution, and I have found $\rho = 1{\cdot}1065$ and $h = 10{\cdot}018$ millimetres. One has consequently $2h\rho = 22{\cdot}17$, a number that differs but little from 22·56, which experiment has furnished me; and the agreement appears still more satisfactory when it is remembered that these two numbers are respectively deduced from elements totally different. The formula

$$p = \frac{2h\rho}{d}$$

may therefore be regarded as clearly verified by experiment.

The accuracy of this formula requires, however, that the film which constitutes the bubble should not have in any of its points a thickness less than twice the radius of appreciable molecular attraction. In fact, the pressure exerted upon the enclosed air is the sum of the two actions due to the curvatures of the faces of the film; and, on the other hand, it is known that in the case of a full liquid mass the capillary pressure of the liquid upon itself emanates from all the points of a superficial stratum having for thickness the radius of activity in question. If, then, in all its points the film has a thickness less than twice this same radius, the superficial layers of its two faces have no longer their complete thickness, and, the number of molecules contained in one of these layers being thus diminished, these same layers must necessarily exert a weaker action; hence the sum of the latter, that is to say the pressures on the enclosed air, must be less than is indicated by the formula.

I shall thence deduce a convenient method which furnishes an approximate value for the radius of activity now under consideration, or at least within a limit extremely little below which this radius is found. If, having inflated a small bubble in the orifice of the funnel of my apparatus, it is enclosed in a small glass globe, it exhibits a remarkable phenomenon; for, after some time, by placing the eye on a level with its centre, one sees a large space, perceptibly circular, coloured with a uniform tint, and surrounded by narrow concentric rings of other colours. One would infer from this that the point has been reached at which the film has thickness appreciably uniform throughout the whole extent of the bubble, except of course the lowest part, where there is always a small accumulation of liquid: the colours of the rings which surround the central part evidently arise from the obliqueness of the rays from them to the eye. This fact respecting thickness has already been noticed by Newton, but only as occurring by chance, in the hemispherical bubbles of soapy water. From the moment the bubble assumes this appearance it main-

tains it till it bursts ; the respective tints of the central space and of the rings, however, vary progressively, changing in the order of the colours of Newton's rings, whence it follows that the film becomes thinner and thinner but equally all over, always excepting the very lowest portion of the bubble.

Now, after the film has acquired a uniform degree of thinness, if the pressure exerted on the enclosed air experienced a diminution, it would be rendered apparent by the manometer, and it would be seen to proceed in a regular manner and in proportion to the further weakening of the film. In this case the thickness of the film, when the diminution of the pressure commenced, could be determined by means of the colour which the central space at that moment presented, and half of this thickness would be the value of the radius of appreciable molecular attraction. If, on the contrary, the pressure continued constant until the bursting of the bubble, one would infer, from the colour of the central space, the final thickness of the film, and the half of this thickness would at least constitute the limit but a very little below that in which is found the radius in question.

I have tried the application of this method. By means of a number of precautions, which I have pointed out in the memoir, a bubble, 2 centimetres in diameter, inflated in the orifice of a small funnel and enclosed in a glass globe, existed for nearly three days, and at the time it burst it had reached the state of transition from yellow to white of the first order. The levels of water in the manometer had made little oscillations during this period, sometimes in one direction, sometimes in another; still the last was indicative of an increase of pressure. For reasons mentioned in the memoir, these oscillations could not be attributed—at least entirely—to variations of temperature, and I have thought it admissible that the continual diminution of the thickness of the film had not brought about any decrease of pressure; consequently the final thickness was most likely more than twice the radius of molecular attraction.

Calculating the final thickness of the film by means of Newton's numbers and the index of refraction of glyceric liquid, an index whose value, previously determined, was 1·377, I have found the thickness in question to be $\frac{1}{880}$ of a millimetre. Half of this quantity, or $\frac{1}{1760}$ of a millimetre consequently constitutes the limit furnished by my experiment; but, to be on the safe side, I prefer $\frac{1}{1700}$.

I have thus arrived at a very probable conclusion, that in the glyceric liquid the radius of appreciable molecular attraction is less than $\frac{1}{1700}$th of a millimetre.

I propose to continue this research in order to investigate the black colour, and to throw light on the question of the variations of the manometer.

XXXVIII. *On the Amount of the direct Magnetic Effect of the Sun or Moon on Instruments at the Earth's Surface. By* G. Johnstone Stoney, *M.A., F.R.S., Secretary to the Queen's University in Ireland**.

IN the Philosophical Magazine for March 1858, Dr. Lloyd showed that the observed disturbances of the magnetic needle, depending on the hours of solar or lunar time, follow laws inconsistent with their being due to the *direct* magnetic attraction of the sun or moon. Hence it might be too hastily concluded, from the absence of observed effects following the proper laws, that these luminaries are not magnetic. An inquiry into the amount of this influence, however, shows that, though the sun or moon were as highly magnetized as the earth, their direct effects would be so small as to be masked by the more powerful unknown perturbating causes which the observations prove to be at work.

In fact let O and O′ be the centres of a distant magnet and of a needle acted on. Let x, y, z be the coordinates of dm, a molecule of the distant magnet referred to O as origin, and rectangular coordinates so taken that the axis of x may pass through O′. Let also x', y', z' be the coordinates of dm', a molecule of the needle acted upon, referred to parallel coordinates passing through O′. Then using ρ for the distance betwen dm and dm', and D for the distance between the centres of the magnets, the components of the action of dm on dm' will be

$$d\mathrm{X} = \frac{dm\,dm'}{\rho^2}\cdot\frac{\mathrm{D}+x'-x}{\rho},$$

$$d\mathrm{Y} = \frac{dm\,dm'}{\rho^2}\cdot\frac{y'-y}{\rho},$$

$$d\mathrm{Z} = \frac{dm\,dm'}{\rho^2}\cdot\frac{z'-z}{\rho}.$$

Therefore the elementary moments turning dm' round O′ will be

$$d\mathrm{P} = \frac{dm\,dm'}{\rho^3}\cdot(yz'-zy'),$$

$$d\mathrm{Q} = \frac{dm\,dm'}{\rho^3}\cdot(zx'-xz'+z'\mathrm{D}),$$

$$d\mathrm{R} = \frac{dm\,dm'}{\rho^3}\cdot(xy'-yx'-y'\mathrm{D}).$$

But $\rho^2=(\mathrm{D}+x'-x)^2+(y'-y)^2+(z'-z)^2$. Therefore, expanding

* Communicated by the Author. An abstract of this paper was read at the recent Manchester Meeting of the British Association.

in inverse powers of D,

$$\frac{1}{\rho^3} = \frac{1}{D^3} \cdot \left(1 - 3\frac{x'-x}{D} + \dots\right).$$

Hence, expanding, rejecting terms in which D^{-4} occurs, and those into which coordinates of both dm and dm' do not enter (since they would disappear in integrating, from the fundamental property of magnetism that $\int dm = 0$),

$$dP = +\frac{dm\,dm'}{D^3} \cdot (yz' - zy') + \text{small terms},$$

$$dQ = +\frac{dm\,dm'}{D^3} \cdot (zx' + 2xz') + \text{rejected terms},$$

$$dR = -\frac{dm\,dm'}{D^3} \cdot (2xy' + yx') + \text{rejected terms}.$$

Let M and M′ be the magnetic moments, and $\alpha\beta\gamma$, $\alpha'\beta'\gamma'$, the directions of the magnetic axes, so that

$$M = (\int x\,dm)^2 + (\int y\,dm)^2 + (\int z\,dm)^2,$$

$$M'^2 = (\int x'\,dm')^2 + (\int y'\,dm')^2 + (\int z'\,dm')^2;$$

$$\cos\alpha = \frac{\int x\,dm}{M}, \quad \cos\beta = \frac{\int y\,dm}{M}, \quad \cos\gamma = \frac{\int z\,dm}{M},$$

$$\cos\alpha' = \frac{\int x'\,dm'}{M'}, \quad \cos\beta' = \frac{\int y'\,dm'}{M'}, \quad \cos\gamma' = \frac{\int z'\,dm'}{M'}.$$

Then integrating, the components of the moment turning the needle round O′ will be

$$P = +\frac{M\,M'}{D^3}(\cos\beta\cos\gamma' - \cos\gamma\cos\beta') + \text{small terms},$$

$$Q = +\frac{M\,M'}{D^3}(\cos\gamma\cos\alpha' + 2\cos\alpha\cos\gamma') + \text{small terms},$$

$$R = -\frac{M\,M'}{D^3}(2\cos\alpha\cos\beta' + \cos\beta\cos\alpha') + \text{small terms}.$$

Hence the resultant moment tending to turn the needle round O′

$$= \frac{MM'}{D^3}\sqrt{(\cos\beta\cos\gamma' - \cos\gamma\cos\beta')^2 + (\cos\gamma\cos\alpha' + 2\cos\alpha\cos\gamma')^2 + (2\cos\alpha\cos\beta' + \cos\beta\cos\alpha')^2} + \text{small terms}.$$

The maximum value of this (neglecting the small terms) is

$2\frac{MM'}{D^3}$, and arises when $\cos\alpha=1$ and $\cos\alpha'=0$*; that is, when the magnetic axis of the distant magnet is pointed towards the needle, and at the same time the needle stands in a perpendicular direction.

Now if H signify the horizontal, and T the total intensity of the earth's magnetism at any station expressed in Gauss's absolute units, $M'H\sin h$ and $M'T\sin t$ will be the moments by which the earth tends to restore the declination and dipping needles respectively, when displaced from their positions of rest through the angles h and t. If we suppose then that the moon is brought successively into the positions in which it will most deviate the two needles, we find that

$M'H\sin h=2\frac{MM'}{D^3}$ is the condition of rest for the declination needle, and

$M'T\sin t=\frac{2MM'}{D^3}$ is the condition of rest for the dipping needle.

Hence the greatest deviations which the moon can produce on the declination and dipping needles respectively will be

$$h=\frac{2M}{D^3H'} \qquad t=\frac{2M}{D^3T'}$$

writing the small angles h and t instead of their sines.

In order to arrive at numerical values, it will be necessary to remember that the magnetic moment M, or

$$\sqrt{\{(\int x\,dm)^2+(\int y\,dm)^2+(\int z\,dm)^2\}},$$

is independent of the position of the origin of coordinates. In fact the moment referring to a new origin abc is

$$\sqrt{\{(\int(x-a)\,dm)^2+(\int(y-b)\,dm)^2+(\int(z-c)\,dm)^2\}},$$

which = M, since, from the fundamental property of magnetism, $\int dm=0$. It is obvious that it is also independent of the direction of the coordinate axes. From this we conclude that in magnetic bodies, since they consist of parts throughout each of

* This may be easily seen by conceiving the force of which the components are $-2\cos\alpha$, $\cos\beta$, and $\cos\gamma$, applied to the point of which the coordinates are $\cos\alpha'$, $\cos\beta'$, $\cos\gamma'$. The radical in the text will then represent the moment of this force round the origin: and bearing in mind that $\cos\alpha'$, $\cos\beta'$, $\cos\gamma'$ are coordinates of a point at a unit distance from the origin, and that $-2\cos\alpha$, $2\cos\beta$, $2\cos\gamma$ would be components of a force equal to 2, it is obvious that the maximum moment of the given force will amount to 2, and will arise when $\cos\alpha=1$ $\cos\alpha'=0$.

which $\int dm = 0$*, the magnetic moment of the whole is the sum of the magnetic moments of its parts; from which it follows that *the magnetic moments of similar bodies*, if equally magnetized in corresponding parts, *are proportional to their volumes*. Therefore, as the action of the moon on our instruments varies as $\frac{M}{D^3}$, we may substitute for the moon a hypothetical globe subtending at our instruments the same angle as the moon, and equally magnetized bulk for bulk. If, then, the moon be as magnetic as the earth, its maximum effect will equal that of a globe one metre in diameter, of materials as magnetic as the earth, and placed at such a distance from the instrument as to subtend an angle of $2043''$, which is the greatest apparent diameter of the moon as seen from the *surface* of the earth.

Now Gauss found the magnetic moment of a steel magnet bar one pound in weight, referred to his absolute unit, to be 100,877,000, and he has shown† that the moment of the earth's magnetism is equal to what would be produced by 7·831 such bars placed parallel to one another in each cubic metre of its volume. Hence the magnetic moment of a cubic metre magnetized in proportion to its bulk as much as the earth is $7{\cdot}831 \times 100{,}877{,}000$; and multiplying this by ·5236, the ratio of the contents of a sphere to the cube of its diameter, we find for the moment of the globe a metre in diameter, expressed in Gauss's absolute units,

$$M = 0{\cdot}5236 \times 7{\cdot}831 \times 100{,}877{,}000.$$

Again, 206264·8 being the number of seconds in radius unity, the distance of the globe, in order to subtend the same angle as the moon when nearest, will be $\frac{206264{\cdot}8}{2043}$ metres, or (to express it in Gauss's unit of length) $\frac{206264800}{2043}$ millimetres.

Also, expressed in the same units, Gauss found the horizontal intensity at Göttingen on the 19th July, 1834, $H = 1{\cdot}7748$, and the total intensity $T = 4{\cdot}7414$. Therefore, finally, the maximum deviations, expressed in seconds, which the moon, if as magnetic bulk for bulk as the earth, could produce at Göttingen were

* This is equivalent to requiring that the *parts* spoken of in the text be formed by divisions so disposed as not to split any magnetic molecule in such a way as would place the north magnetism it contains in one part and the south magnetism in another. All fractures which can in practice be effected, fulfil this condition.

† See Gauss's Memoir "On the General Theory of Terrestrial Magnetism," translated in 'Taylor's Scientific Memoirs.'

$$h=\frac{2\times 0{\cdot}5236\times 7{\cdot}831\times 100877000\times 206264{\cdot}8}{\left(\frac{206264800}{2043}\right)^3\times 1{\cdot}7748},$$

which is less than $0''{\cdot}094$, and

$$t=\frac{2\times 0{\cdot}5236\times 7{\cdot}831\times 100877000\times 206264{\cdot}8}{\left(\frac{206264800}{2043}\right)^3\times 4{\cdot}7414},$$

which is less than $0''{\cdot}036$. *Hence at Göttingen the direct disturbance of the instrument of declination does not amount, at its maximum, to one-tenth of a second of arc, and that of the dip circle does not reach even one twenty-seventh of a second.*

Now the observations with which these should be compared have been made at several stations. The principal part of the observed lunar diurnal variation consists of a term depending on twice the lunar hour-angle, but there is also a small term containing the simple hour-angle. This latter is the one which, as Dr. Lloyd has shown, the direct action of the moon would affect, and General Sabine* has determined the following values for its coefficient, in calculating the formulæ which would best represent the observations at the several stations:—

$-1''{\cdot}05$ at Toronto,
$+0{\cdot}88$ at St. Helena,
$+1{\cdot}21$ at the Cape,
$+0{\cdot}97$ at Hobarton,
$-0{\cdot}81$ at Pekin, and
$-2{\cdot}04$ at Kew

for the declination; and—

$-1''{\cdot}14$ at Toronto,
$+1{\cdot}32$ at St. Helena,
$-0{\cdot}94$ at the Cape, and
$-0{\cdot}48$ at Hobarton

for the inclination.

There is then no ground for presuming, from the minuteness of the coefficient, that the moon is not of as magnetic or even much more magnetic materials than the earth. On the contrary, the actual magnitudes of the coefficients are *too large* to be with probability attributed solely to the direct effect of the moon, even if it were not evident from other considerations, that some cause acting by different laws has contributed the greater part to them.

If the comparison with the earth be made mass for mass instead of bulk for bulk, the above disturbances must be reduced

* See p. cxlvi of the Introduction to the 2nd volume of the St. Helena Observations.

in the ratio of the moon's density to that of the earth, that is, to about $\frac{2}{3}$rds of the values already given.

The same method of course applies equally to the sun; and whether his magnetism be regarded as exceeding that of the earth in proportion to his mass or to his bulk, his maximum influence will be even less than that of the moon; for he never attains an apparent size as great as the maximum of the moon, and his density is only about half that of the moon.

XXXIX. *Chemical Notices from Foreign Journals.* *By* E. Atkinson, *Ph.D., F.C.S.*

[Continued from p. 143.]

FROM the readiness with which, in the vegetable kingdom, the oxygen in carbonic acid is replaced by hydrogen, it was highly probable that carbonic acid could similarly be reduced artificially, and that the first product of substitution (formic acid) could be prepared from carbonic acid. Led by these considerations, Kolbe and Schmitt* undertook an investigation on the direct conversion of carbonic acid into formic acid, and their first experiments have been successful. The change succeeds so easily and in such a simple manner as to make it surprising that it has not been previously observed. When potassium was spread out in a thin layer on a flat dish, and this was placed under a bell-jar standing over milk-warm water, and kept continually filled with carbonic acid, the potassium was found in twenty-four hours to be converted into a mixture of bicarbonate and of formiate of potash. The reaction may be thus written:—

$$2\,K + 2C^2\,O^4 + 2\,HO = KO,\,C^2\,HO^3 + \left.\begin{matrix} KO \\ HO \end{matrix}\right\} C^2\,O^4.$$

The above mixture was supersaturated in the cold with sulphuric acid, the acid liquor poured off from the bisulphate of potash distilled, and the distillate neutralized with carbonate of lead. On evaporating the hot filtered solution, chemically pure formiate of lead was obtained.

Sodium exposed for twenty-four hours to the action of carbonic acid and aqueous vapour, also gives rise to the formation of formic acid, but in smaller quantity than potassium.

Schischkoff, in continuing his researches on nitroform†, has obtained results of which he communicates a preliminary notice‡.

* Liebig's *Annalen*, August 1861.
† Phil. Mag. vol. xv. p. 302.
‡ Liebig's *Annalen*, August 1861.

He finds that nitroform, $C(NO^2)^3H$, is a strong acid, and readily exchanges its atom of hydrogen for metals, forming true salts. This hydrogen can also be replaced by bromine and by hyponitrous acid. When nitroform, mixed with bromine, was exposed to the sunlight, hydrobromic acid was formed, and the mixture became decolorized. The resultant product was washed with water, in which it is somewhat soluble; it is liquid at temperatures above +12° C., but below that point solidifies to a crystalline mass. It has the formula $C(NO^2)^3Br$.

In order to replace the hydrogen in nitroform by hyponitrous acid, a current of air was passed through a mixture of nitroform with sulphuric and nitric acids heated to 100°. A liquid distilled over, from which, on the addition of water, an insoluble oily liquid was precipitated. This substance boils at 126° C. without any decomposition; it is colourless, mobile, and fluid at ordinary temperatures, but solidifies at +13° C. to a white crystalline mass. It has the composition $C(NO^2)^4$, and singularly enough, although it contains an atom more hyponitrous acid, it is more stable than nitroform; it does not explode when rapidly heated, but decomposes, giving off nitrous vapours.

Schischkoff had found that trinitroacetonitrile, $C^2(NO^2)^3N$, was decomposed by sulphuretted hydrogen, yielding a body $C^2(NO^2)^2(NH^4)N$, which he called binitroammonyle. This he has since found* to be the ammonium-salt of the body *binitroacetonitrile*, $C^2(NO^2)^2HN$, which has strongly acid properties. Binitroacetonitrile is obtained by treating an aqueous solution of binitroammonyle with sulphuric acid and agitating the mixture with ether. On evaporating the etherial solution, the acid crystallizes in large colourless plates. The silver and potassium salts of this body were prepared. The silver-salt has the formula $C^2(NO^2)^2AgN$; when treated with bromine in presence of water, bromide of silver is formed, and an oily product, which is probably bromobinitroacetonitrile, $C^2(NO^2)^2BrN$.

A series of experiments by Wurtz and Friedel on lactic acid† confirm the conclusion‡ that lactic acid contains a diatomic radical, and that its two equivalents of replaceable hydrogen are not of identical value.

There are two ethers of lactic acid which are isomeric, but completely different in properties. One of them, ethylactic acid, is obtained by treating dilactate of ethyle with caustic potash; the other is neutral, and was first obtained by Strecker in distilling lactate of lime with sulphovinate of potash. Wurtz

* Liebig's *Annalen*, August 1861.
† *Comptes Rendus*, May 27, 1861.
‡ Phil. Mag. vol. xviii. p. 287.

and Friedel have found that it is also obtained by heating lactic acid with alcohol in closed vessels to a temperature of 170°. The former of these compounds is a true acid, and readily forms salts. When monoethylic lactate, as the latter compound is called, is treated with potassium, hydrogen is disengaged, and a compound is obtained isomeric with ethylactate of potash, and which, when treated with iodide of ethyle, forms dilactic ether.

These two ethers present a most curious example of isomerism. They are formed by the same acid, both contain the same group ethyle, and yet one of them is an energetic acid, while the other is perfectly neutral. This is accounted for by the different parts that the two atoms play in lactic acid. One of them is strongly basic, and can be replaced by a metal or by an organic group, such as ethyle; in both cases a neutral compound is obtained. The other atom can be easily replaced by oxygen groups, such as the radicals of monobasic acids. If replaced by an indifferent group, such as ethyle, it is still acid, because the atom of basic hydrogen has not been touched.

The authors have noted similar isomeric relations between lactamethane, $C^5H^{11}NO^2$, and a new amide produced by the action of ethylamine on lactide, $C^3H^4O^2$. By potash they undergo a different decomposition—the latter into lactic acid and ethylamine, and the former into ammonia and ethylactic acid.

Lactyle, the radical of lactic acid, has the property of multiplying itself in one and the same body so as to form compounds which may be referred to condensed types analogous to the polyethylenic compounds.

Dilactic Ether.—When chlorolactic ether* acts upon lactate of potash, chloride of potassium is formed, and a dilactic ether, according to the equation

$$C^5H^9O^2Cl + C^3H^5KO^3 = KCl + C^8H^{14}O^5.$$

Chlorolactic ether. Lactate of potassium. New body.

The formula of the new body, *monoethylic dilactate*, may be written thus, $\left.\begin{matrix}(C^3H^4O'')^2\\(C^2H^5)H\end{matrix}\right\}O^3$. Besides this there is another lactic ether, *diethylic dilactate*, $\left.\begin{matrix}(C^3H^4O'')^2\\(C^2H^5)^2\end{matrix}\right\}O^3$, obtained by the action of chlorolactic ether on ethylolactate of potassium. These compounds are the ethers of the anhydrous lactic acid of Pelouze, and can be regarded as containing two equivalents of the radical lactyle according to the formula $\left.\begin{matrix}(C^3H^4O'')\\H^2\end{matrix}\right\}O^3$. There is a lac-

* Phil. Mag. vol. xviii. p. 287.

tosuccinic ether, $\mathrm{C^9\,H^{18}\,O^6}$, a mixed ether obtained by treating chlorolactic ether with ethylosuccinate of potash. It has the formula $\left.\begin{matrix}\mathrm{C^3\,H^4\,O''}\\ \mathrm{C^4\,H^4\,O^{2\prime\prime}}\\ \mathrm{(C^2\,H^5)^2}\end{matrix}\right\}\mathrm{O^3}$. There is, further, a trilactic ether, formed by the direct union of lactide with lactic ether.

By the action of sodium-alcohol on iodoform, Boutlerow obtained, among other products, an acid which he believed was valerolactic acid. He has since found* that this acid is ethylactic acid; for when treated with hydriodic acid it is decomposed into lactic acid and iodide of ethyle,

$$\underset{\text{New acid.}}{\mathrm{C^5\,H^{10}\,O^3}} + \mathrm{H\,I} = \underset{\text{Lactic acid.}}{\mathrm{C^3\,H^6\,O^3}} + \underset{\text{Iodide of ethyle.}}{\mathrm{C^2\,H^5\,I}}.$$

He has also proved by direct experiments that this acid is identical in properties with Wurtz's ethylactic acid, obtained by the decomposition of lactic ether by potash.

Vogt has given a fuller account† of the preparation and properties of the new benzylic mercaptan which he discovered, and of which a preliminary notice has already appeared‡; he has also described a series of its compounds. The sodium-benzylic mercaptan, $\mathrm{C^{12}\,H^5\,Na\,S^2}$, is obtained as a white saline mass by the addition of sodium to benzyle-mercaptan and subsequent evaporation to dryness. The lead compound, $\mathrm{C^{12}\,H^5\,Pb\,S^2}$, a yellow crystallized body, is obtained by adding an alcoholic solution of acetate of lead to an alcoholic solution of the mercaptan. The mercury compound, $\mathrm{C^{12}\,H^5\,Hg\,S^2}$, crystallizes in very fine white needles, and is obtained by the action of oxide of mercury on the mercaptan.

Nitric acid acts with considerable energy on benzyle-mercaptan. The result of this action is a body crystallizing in white lustrous needles, which has the formula $\mathrm{C^{12}\,H^5\,S^2}$, and is accordingly *bisulphide of benzyle*; its formation may be thus expressed:—

$$\underset{\text{Benzylic mercaptan.}}{\mathrm{C^{12}\,H^6\,S^2}} + \mathrm{NO^5\,HO} = \underset{\text{Bisulphide of benzyle.}}{\mathrm{C^{12}\,H^5\,S^2}} + \mathrm{NO^4} + 2\,\mathrm{HO}.$$

It has a faint but not unpleasant smell, and melts at 60° to a yellowish oil, which can be distilled at a high temperature without decomposition.

* Liebig's *Annalen*, June 1861.
† Ibid. August 1861.
‡ Phil. Mag. vol. xx. p. 522.

Bisulphide of benzyle can readily be reduced to benzyle-mercaptan by nascent hydrogen.

Another remarkable and hitherto unexplained mode of forming bisulphide of benzyle, by which it may be obtained in large transparent crystals, consists in dissolving the mercaptan in alcoholic ammonia and exposing the solution to spontaneous evaporation.

By the further oxidation of benzylic mercaptan, benzyle-sulphuric acid is formed, $HO\,C^{12}H^5S^2O^5$.

Mosling has investigated* the action of hydrochloric acid and of sulphuretted hydrogen on benzoic anhydride. The action of the former substance is simply in accordance with the equation

$$\left.\begin{matrix}C^7H^5O\\C^7H^5O\end{matrix}\right\}O + \left.\begin{matrix}H\\Cl\end{matrix}\right\} = \left.\begin{matrix}C^7H^5O\\H\end{matrix}\right\}O + \left.\begin{matrix}C^7H^5O\\Cl\end{matrix}\right\}$$

Benzoic anhydride. — Benzoic acid. — Chloride of benzoyle.

Benzoic anhydride was heated with sulphuretted hydrogen to a temperature of 130° for twenty hours. Some benzoic acid sublimed, and the residue in the retort, when crystallized from alcohol and bisulphide of carbon, was found to consist of a new body, which had the formula

$$C^7H^5O\,S \text{ or } \left.\begin{matrix}C^7H^5O\\C^7H^5O\end{matrix}\right\}S^2,$$

and is therefore the *persulphide of benzoyle*. It is not soluble in water, and difficultly so in alcohol.

It readily dissolves in ether, and especially in bisulphide of carbon, from which it crystallizes in colourless plates which appear to be rhombic columns. It melts at 123°, and decomposes at a somewhat higher temperature. It is the first member of a new series of sulphur-compounds, and corresponds to Brodie's peroxide of benzoyle and acetyle.

It is probably formed in accordance with the following reaction:—

$$3\left.\begin{matrix}C^7H^5O\\C^7H^5O\end{matrix}\right\}O + 2\left.\begin{matrix}H\\H\end{matrix}\right\}S = 3\left.\begin{matrix}C^7H^5O\\H\end{matrix}\right\}O + \left.\begin{matrix}C^7H^5O\\C^7H^5O\end{matrix}\right\}S^2 + \left.\begin{matrix}C^7H^5O\\H\end{matrix}\right\}$$

Benzoic anhydride. — Benzoic acid. — Persulphide of benzoyle. — Hydride of benzoyle.

In a preliminary notice, Kalle† announced that, by the action of zinc-ethyle on chloride of sulphon-benzyle, he had obtained a new body which was a mixed acetone belonging to the benzyle

* Liebig's *Annalen*, June 1861.

† Phil. Mag. vol. xx. p. 522.

series, but containing sulphur in the place of some of the carbon. A subsequent examination* of the reaction has shown that chloride of ethyle is formed at the same time, along with the zinc-salt of a new acid. The reaction may be thus expressed :—

$$(C^{12}H^5)[S^2O^4]Cl + Zn\,C^4H^5 = ZnO\,C^{12}H^5S^2O^3 + C^4H^5Cl.$$

Chloride of sulphon-benzyle. — Zinc-ethyle. — Benzyle-sulphite of zinc. — Chloride of ethyle.

Benzyle-sulphurous acid, the product of this reaction, stands to sulphurous acid in the same relation as benzyle-sulphuric acid to sulphuric acid; it is sulphurous acid $(S^2O^2)O^2$ in which an atom of oxygen is replaced by benzyle. It crystallizes in large prisms, often an inch long, and mostly occurring in stellate groups.

The author describes several of the salts of the new acid, and also a series of experiments made with the view of finding a more productive method of its preparation.

Wurtz has continued† his researches on the oxyethylenic bases formed by the action of oxide of ethylene on ammonia. The product of this action, when treated by hydrochloric acid, consists mainly of the hydrochlorates of trioxethylenamine, $(C^2H^4O)^3NH^3$, and dioxethylenamine, $(C^2H^4O)^2NH^3$. The former is insoluble; and from the alcoholic mother-liquor, the platinum-salt of the second, $C^4H^{11}NO^2$, $HCl\,PtCl^2$, is precipitated on the addition of bichloride of platinum. On the addition of ether to the mother-liquor, the platinum-salt of a third base, monoxethylenamine, is precipitated. It has the formula $(C^2H^4O)\,HCl$, $PtCl^2$, and crystallizes in golden-yellow nacreous laminæ.

The hydrochlorates of monoxethylenamine and of dioxethylenamine are formed by the action of ammonia on hydrochloric glycol when these substances, enclosed in strong vessels, are heated in the water bath.

$$C^2H^5ClO + NH^3 = (C^2H^4O)\,NH^3, HCl.$$

Hydrochlorate of monoxethylenamine.

$$2(C^2H^5ClO) + 2NH^3 = 2(C^2H^4O^2)\,NH^3, HCl + NH^4Cl$$

Hydrochlorate of dioxethylenamine.

The base trioxethylenamine may be isolated and obtained as a thick syrup by the action of oxide of silver on its hydrochlorate.

* Liebig's *Annalen*, August 1861.

† *Comptes Rendus*, August 19, 1861, Phil. Mag. vol. xix. p. 125.

When this base is heated with hydrochloric glycol, the hydrochlorate of the base tetroxethylenamine is formed.

$$(C^2H^4O)^3NH^3 + C^2H^5ClO = (C^2H^4O)^4NH^3, HCl.$$

Trioxethylenamine. Hydrochloric glycol. Hydrochlorate of tetroxethylenamine.

With reference to the constitution of these bases, if it be assumed that the diatomic oxide of ethylene, by fixing an atom of hydrogen in ammonia, may become monatomic,

$$C^2H^4O'' + H = C^2H^5O',$$

they may be referred to the type ammonia, and the formulæ of their hydrochlorates become,—

$\left.\begin{matrix}C^2H^5O' \\ H^3\end{matrix}\right\}$NCl Hydrochlorate of monoxethylenamine.

$\left.\begin{matrix}(C^2H^5O)^2 \\ H^2\end{matrix}\right\}$NCl Hydrochlorate of dioxethylenamine.

$\left.\begin{matrix}(C^2H^5O)^3 \\ H\end{matrix}\right\}$NCl Hydrochlorate of trioxethylenamine.

$(C^2H^5O)^4$ } NCl Hydrochlorate of tetroxethylenamine.

But they may also be referred to the mixed type, water and ammonia, $\left.\begin{matrix}nH^2 \\ H^3\end{matrix}\right\}\begin{matrix}O^n \\ N\end{matrix}$; and the author prefers this view.

The union of anhydrous trioxethylenamine with ammonia can take place in several proportions. One, two, three, or four molecules of oxide of ethylene can unite with one molecule of the anhydrous base, forming oxygenated bases more and more complex, but in which the basic power is also feebler. They nevertheless have an alkaline reaction, combine with hydrochloric acid, and form double salts with bichloride of platinum. These latter do not crystallize, and are very difficult to purify and separate. The analysis of some of these bases gave results agreeing with the formulæ

$$(C^2H^4O)^5NH^3.HCl, PtCl^2,$$
$$(C^2H^4O)^7NH^3.HCl, PtCl^2.$$

These bodies, although containing nitrogen, and being distinctly alkaline, are not compound ammonias, and cannot be referred to that type. It is accordingly probable that among natural oxygen bases there are some which are not compound ammonias, that is, cannot be regarded as derived from ammonia by substitution.

Wurtz* tried the action of aldehyde on glycol, expecting to

* *Comptes Rendus*, August 26, 1861.

obtain a series of bodies isomeric with the polyethylenic alcohols; the reaction, however, is quite different: the aldehyde dehydrates glycol, and unites with the oxide of ethylene thus formed:—

$$C^2 H^6 O^2 + C^2 H^4 O = C^4 H^8 O^2 + H^2 O.$$

Glycol. Aldehyde. New body.

It is a colourless limpid liquid, with an agreeable penetrating odour, resembling that of aldehyde. It boils at 82°·5.

If aldehyde is the oxide of ethylidene, the compound is a mixed oxide of ethylene-ethylidene. The body slowly reduces alcoholic solution of nitrate of silver. Heated with acetic acid, it regenerates diacetate of glycol.

Kekulé has published* an interesting communication on fumaric and some allied organic acids. When malic acid, $C^4 H^6 O^5$, is heated, it loses water, and gives two isomeric bodies, fumaric and maleic acids, $C^4 H^4 O^4$.

When fumaric acid is treated with bromine in the presence of water, no action takes place in the cold, but at the temperature of the water-bath the bromine rapidly disappears, and a quantity of perfectly white crystals are obtained, which are dibromosuccinic acid, $C^4 H^4 Br^2 O^4$. The formation of this body is interesting, inasmuch as it takes place by a simple *addition* of the elements, and not, as is usually the case in the action of bromine on organic substances, by *substitution*: thus

$$C^4 H^4 O^4 + Br^2 = C^4 H^4 Br^2 O^4$$

Fumaric acid. Dibromosuccinic acid.

Hydrobromic acid also, when heated with fumaric acid, yields some monobromosuccinic acid, but the action is very slow.

$$C^4 H^4 O^4 + HBr = C^4 H^5 Br O^4$$

Fumaric acid. Monobromosuccinic acid.

Fumaric acid can also be converted into succinic acid by the action of hydrogen. The experiment succeeds by means of hydriodic acid, but is most easily effected by means of nascent hydrogen. It is simply necessary to add sodium-amalgam to a solution of fumaric acid in water to convert it entirely into succinic acid.

$$C^4 H^4 O^4 + H^2 = C^4 H^6 O^4$$

Fumaric acid. Succinic acid.

This action of nascent hydrogen is as unusual as that of bromine. Hydrogen in the nascent state can reduce organic substances by taking away oxygen; but there are few cases in which an organic substance unites directly with hydrogen.

* Liebig's *Annalen*, Supplement, July 1861.

Kekulé has found that maleic acid, when acted upon by the same reagents, yields the same bodies.

In conclusion, he developes his views as to the relations between fumaric acid and its allied substances. He establishes a close and interesting analogy between fumaric acid and ethylene. Fumaric acid stands in the same relation to malic acid as ethylene does to alcohol; it stands to dibromosuccinic acid as ethylene does to bromide of ethylene; and to monobromosuccinic acid as ethylene to bromide of ethyle, and so on. Tartaric acid is to fumaric acid what glycol is to ethylene. In fact tartaric acid is obtained when the bromide of fumaric acid, that is, dibromosuccinic acid, is heated with oxide of silver, just as an ether of glycol is obtained when a silver salt acts upon bromide of ethylene.

In Poggendorff's *Annalen* for 1855, Kessler described a volumetric method of estimating arsenic and antimony, by which he made a determination of their atomic weights. The method consisted in oxidizing these substances, which were employed in the form of arsenious and antimonious acids, to arsenic and antimonic acids by means of a standard solution of bichromate of potash; the excess of bichromate of potash was determined by means of a standard solution of protochloride of iron. The applicability of this method depends upon the fact that protochloride of iron reduces bichromate of potash, but does not affect arsenic or antimonic acid. Kessler has since then made * some additional experiments, partly confirming and partly rectifying previous results.

From these experiments he concludes that the atomic weight of arsenic is 75·15.

For the atomic weight of antimony, Kessler's previous experiments led to the number 123·78. In his recent experiments, in which some sources of error, to which his previous methods were liable, have been avoided, he has obtained different results.

Pure oxide of antimony was prepared, and was further purified by sublimation in a porcelain tube in a current of carbonic acid; a given weight of this was partially oxidized by a given weight of pure chlorate of potash, and the oxidation completed by means of a standard solution of bichromate of potash; the excess of the latter was estimated by a standard solution of protochloride of iron. In this way six experiments gave numbers for the equivalent of antimony, varying between 121·67 and 122·58, the mean being 122·16.

In another case, in which pure metallic antimony was oxi-

* Poggendorff's *Annalen*, vol. cxiii. p. 134.

dized to antimonic acid by bichromate of potash, the number found was 122·34.

In a third case, a double determination of terchloride of antimony was made, by oxidation to pentachloride, and by directly determining the quantity of chlorine in the ordinary way. This gave the number 122·37 for the equivalent of antimony.

The mean of these results obtained by different methods is 122·29; they furnish a remarkable confirmation of the excellent determinations of Dexter. The method employed by this chemist was that originally used by Berzelius, and consisted in the direct oxidation of pure antimony to antimoniate of oxide of antimony, SbO^4. By numerous very careful experiments he obtained the mean number 122·33.

In the above series of experiments Kessler obtained the number 26·1 for the atomic weight of chromium.

De Luca describes* the following method of preparing oxygen which he has used for some time; it only differs in manipulatory details from that of Deville and Debray†. A tubulated retort is filled three-quarters full with pumice and concentrated sulphuric acid, and luted on to a porcelain tube by means of a mixture of asbestos and clay. The tube also contains pumice; it is heated to redness, and the vapour of sulphuric acid passed over it. The oxygen is disengaged with regularity, and is easily purified; in one operation 2 ounces of acid furnished about a gallon and a quarter of gas. The process is analogous to that in which hydrogen is prepared by decomposing water by iron; and it is not more difficult.

Lapschin and Tichanowitsch‡ have made a series of experiments on the electrolysis of organic and other substances, in which they had at their disposal a battery of 1000 elements.

Salicine is decomposed by the battery; the first stage appeared to be its decomposition into grape-sugar and saligenine. On the zinc pole gases were disengaged which were not collected; the next stage appeared to be that the saligenine was oxidized successively to hydride of salicyle and to salicylic acid.

The action of a battery of 900 elements produced in *crystallized acetic acid* a rapid disengagement of gas at the carbon pole, consisting of carbonic acid and carbonic oxide. A very slight quantity of gas was disengaged at the zinc pole, which, however, was lost; at the same time an amorphous mass of carbon was deposited.

* *Comptes Rendus*, July 22.

† Phil. Mag. vol. xxi. p. 295.

‡ *Bulletin de l'Académie de St. Pétersbourg*, vol. iv. p. 80.

Absolute alcohol was almost unaffected by a battery of 900 elements; the quantity of gas collected after seven hours was so small that it could not be analysed. At first there was no action, and a slight action was only set up after some time, when the alcohol had attracted moisture by standing. It may be assumed, therefore, that absolute alcohol offers a complete resistance to the current.

Ether was unattacked by the action of a battery of 900 elements, even when the poles were only a millimetre apart. There was no disengagement of gas; and the boiling-point remained the same as before. When the electrodes were 20 millims. asunder, there was an undulatory motion of the liquid from the carbon to the zinc pole.

With *amylic alcohol* 900 elements were used, the electrodes being 1 millim. apart. The multiplier stood at 20°. There was an undulatory motion from the carbon to the zinc pole, and after some time a yellowish deposit was formed on the zinc pole, which, under the microscope, was seen to consist of a pulverulent mass of yellow colour. When, subsequently, the electrodes were brought nearer, they melted together, the liquid became heated, and a black carbonaceous mass was deposited.

Valerianic acid, *turpentine*, and *anhydrous boracic acid* were unacted upon by a battery of 900 elements.

950 elements produced no action on *bisulphide of carbon*; the multiplier stood at 0°. A previous experiment in 1858 with 800 elements gave an equally negative result.

Silicic acid in the pulverulent form was placed in a clay crucible and exposed to the action of the current. At first there was no action, but afterwards the whole mass became ignited; the side of the crucible nearest the zinc pole was perforated, and a platinum globule melted through, which was found to contain silicon.

300 elements produced no action on dry powdered *oxide of antimony*, nor did 370 on *oxychloride of antimony*.

With dried powdered *oxide of zinc* 370 elements produced an energetic action, and the reduced zinc became ignited. The decomposition also ensued with 60 elements; even with 20 there was a slight action.

40 elements acted strongly on *sulphuret of antimony*; sulphur was liberated at the charcoal pole and became ignited.

Realgar required 260 elements for its decomposition; the products of the action, sulphur and arsenic, immediately took fire, and were converted into sulphurous and arsenious acids.

XL. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 246.]

November 15, 1860.—Major-General Sabine, R.A., Treasurer and Vice-President, in the Chair.

THE following communication was read :—

"On the Laws of the Phenomena of the larger Disturbances of the Magnetic Declination in the Kew Observatory: with notices of the progress of our knowledge regarding the Magnetic Storms." By Major-General Edward Sabine, R.A., Treas. and V.P.

The laws manifested by the mean effects of the larger magnetic disturbances (regarded commonly as effects of magnetic storms) have been investigated at several stations on the globe, being chiefly those of the British Colonial Observatories; but hitherto there has been no similar examination of the phenomena in the British Islands themselves. The object of the present paper is to supply this deficiency, as far as one element, namely the declination, is concerned, by a first approximation derived from the photographs in the years 1858 and 1859, of the self-recording declinometer of the observatory of the British Association at Kew; leaving it to the photographs of subsequent years to confirm, rectify, or render more precise the results now obtained by a first approximation. The method of investigation is simple, and may be briefly described as follows :—

The photographs furnish a continuous record of the variations which take place in the direction of the declination-magnet, and admit of exact measurement in the two relations of time, and of the amount of departure from a zero line. From this automatic record, the direction of the magnet is measured at twenty-four equal intervals of time in every solar day, which thus become the equivalents of the "hourly observations" of the magnetometers in use at the Colonial Observatories. These measures, or hourly directions of the magnet, are entered in monthly tables, having the days of the month in successive horizontal lines, and the hours of the day in vertical columns. The "means" of the entries in each vertical column indicate the mean direction of the magnet at the different hours of the month to which the table belongs, and have received the name of "First Normals." On inspecting any such monthly table, it is at once seen that a considerable portion of the entries in the several columns differ considerably from their respective means or first normals, and must be regarded as "disturbed observations." The laws of their relative frequency, and amount of disturbance, in different years, months and hours, are then sought out, by separating for that purpose a sufficient body of the most disturbed observations, computing the amount of departure in each case from the normal of the same month and hour, and arranging the amounts in annual, monthly, and hourly tables. In making these computations, the first normals require to be themselves corrected, by the omission in each vertical column of the entries noted as disturbed, and by taking fresh means, representing the normals of each month and hour after this omission, and therefore uninfluenced by the larger disturbances. These new

means have received the name of "Final Normals," and may be defined as being the mean directions of the magnet in every month and every hour, after the omission from the record of every entry which differed from the mean a certain amount either in excess or in defect.

In this process there is nothing indefinite; and nothing arbitrary save the assignment of the particular amount of difference from the normal which shall be held to constitute the measure of a large disturbance, and which, for distinction sake, we may call "*the separating value*." It must be an amount which will separate a sufficient body of disturbed observations to permit their laws to be satisfactorily ascertained; but in other respects its precise value is of minor significancy; and the limits within which a selection may be made, without materially affecting the results, are usually by no means narrow; for it has been found experimentally on several occasions, that the *Ratios* by which the periodical variations of disturbance in different years, months and hours are characterized and expressed, do not undergo any material change by even considerable differences in the amount of the separating value. The separating value must necessarily be larger at some stations than at others, because the absolute magnitude of the disturbance-variation itself is very different in different parts of the globe, as well as its comparative magnitude in relation to the more regular solar-diurnal variation; but it must be a *constant* quantity throughout at one and the same station, or it will not truly show the relative proportion of disturbance in different years and different months.

The strength of the Kew establishment being insufficient for the complete work of a magnetic observatory, the tabulation of the hourly directions from the photographic records has been performed by the non-commissioned officers of the Royal Artillery, employed under my direction at Woolwich, where this work has been superintended by Mr. John Magrath, the principal clerk, as have been also the several reductions and calculations, which have been made on the same plan as those of the Colonial Observatories.

In the scale on which the changes of direction of the declination-magnet are recorded in the Kew photographs, one inch of space is equivalent to 22′·04 of arc. On a general view and consideration of the photographs during 1858 and 1859, 0·15 inch, or 3′·31 of arc appeared to be a suitable amount for the separating value to be adopted at that station; consequently every tabulated value which differed 3′·31 or more, either in excess or defect from the final normal of the same month and hour, has been regarded as one of the larger disturbances, and separated accordingly. The number of disturbed observations in the two years was 2424 (viz. 1211 in 1858, and 1213 in 1859), being between one-seventh and one-eighth of the whole body of hourly directions tabulated from the photographs, of which the number was 17,319. The aggregate value of disturbance in the 2424 observations, was 14,901 minutes of arc; of which 7207 minutes were deflections of the north end of the magnet to the west, and 7694 to the east; the easterly deflections thus having a slight preponderance. The number of the disturbed observations, as well as their aggregate values, approximated very closely in each of the two years, 1859 being very slightly in excess. The *decennial period*

of the magnetic storms, indicated by the observations at the British Colonial Observatories between 1840 and 1850, had led to the anticipation that the next epoch of maximum of the cycle might take place in the years 1858–1859. The nearly equal proportions in which the numbers and aggregate values of the larger disturbances took place in 1858 and 1859 are so far in accordance with this view. Should the records of the succeeding years at Kew, made with the same instruments, and examined by the same method, show decreasing disturbance in 1860 and 1861, the precise epoch of the maximum indicated by the records of the Kew declinometer will be "the end of 1858 or commencement of 1859."

In Table I. are shown the aggregate values of disturbance in the two years, arranged under the several hours of solar time in which they occurred. They are also divided into the two categories of westerly and easterly deflections, since the experience gained at other stations has now fully established that the westerly and easterly disturbance-deflections are characterized in all parts of the globe by distinct and dissimilar laws. The Ratios are also shown which the aggregate values at the different hours, both of the westerly and the easterly deflections, bear to their respective mean values,—or, in other words, to the sums respectively of the westerly and easterly deflections at all the hours, divided by 24, and taken as the respective units.

Table I.—Showing the aggregate values of the larger disturbances of the Declination at the different hours of solar time in 1858 and 1859, derived from the Kew Photographs; with the Ratios of disturbance at the several hours to the mean hourly value taken as the Unit.

Mean astronomical hours.	Westerly deflections.		Easterly deflections.		Mean civil hours.
	Aggregate values. (Minutes of arc.)	Ratios.	Aggregate values. (Minutes of arc.)	Ratios.	
18	553·9	1·85	118·9	0·37	6 A.M.
19	549·3	1·83	120·9	0·38	7 A.M.
20	442·9	1·48	115·2	0·36	8 A.M.
21	370·1	1·23	121·2	0·38	9 A.M.
22	376·9	1·26	104·6	0·33	10 A.M.
23	361·8	1·21	125·8	0·39	11 A.M.
0	413·7	1·38	173·0	0·54	Noon.
1	431·1	1·44	153·3	0·48	1 P.M.
2	459 8	1·53	173·0	0·54	2 P.M.
3	513·0	1·71	108·4	0·34	3 P.M.
4	403·9	1·35	141·0	0·44	4 P.M.
5	343·8	1·15	164·8	0·51	5 P.M.
6	282·5	0·94	291·1	0·91	6 P.M.
7	110·7	0·37	381·8	1·19	7 P.M.
8	65·6	0·22	499·0	1·56	8 P.M.
9	88·2	0·29	572·9	1·79	9 P.M.
10	59·0	0·20	724·3	2·25	10 P.M.
11	35·7	0·12	767·8	2·38	11 P.M.
12	146·7	0·49	709·5	2·21	Midnight.
13	141·8	0·47	634·8	1·98	1 A.M.
14	146·7	0·49	577·2	1·80	2 A.M.
15	151·5	0·51	464·8	1·45	3 A.M.
16	289·5	0·97	305·8	0·95	4 A.M.
17	458·9	1·53	144·9	0·45	5 A.M.
Mean hourly value 299·0 = 1·00			Mean hourly value 320·6 = 1·00		

The westerly and easterly deflections in the British Islands, as represented by the automatic records at Kew, are obviously governed, as in all other parts of the globe where the phenomena have been analysed, by distinct laws. The westerly deflections have their chief prevalence from 5 A.M. to 5 P.M., or during the hours of the *day*; the easterly deflections, on the other hand, prevail chiefly during the hours of the *night*, the ratios being above unity from 7 P.M. to 3 A.M., and below unity at all other hours. The easterly have one decided maximum, viz. at 11 P.M., towards which they steadily and continuously progress from 5 P.M., and from which they as steadily and continuously recede until 5 A.M. the following morning. The westerly deflections appear to have two epochs of maximum, one from 6 to 7 A.M., the other about 3 P.M., progressing regularly towards the first named from 3 A.M., and receding from it to 9 A.M.; at 9, 10, and 11 A.M. the ratios remain almost sensibly the same, but towards noon they begin to increase afresh, and continue to do so progressively to the second maximum at 3 P.M., from which hour they progressively decrease to 7 P.M. Those ratios which are less than unity, viz. those of the westerly deflections from 6 P.M. to 4 A.M., and of the easterly from 4 A.M. to 6 P.M., do not in either case exhibit the same decided tendency to one or two well-marked minima, as the ratios which are above unity do in both cases towards their maxima. It is possible, however, that this may in some degree be explained by the following consideration:—

The aggregate values of the disturbances prevailing at the different hours, as stated in the Table, are those which have prevailed, not only over the forces which would retain the magnet in its *mean* position, but also over any disturbing influences in an opposite direction, which may be conceived to have existed contemporaneously; and we cannot but suppose that as both westerly and easterly disturbances do record themselves as prevailing at the same hours on different days, that these opposite influences may sometimes *coexist*, neutralizing each other and not appearing in the record. We may reasonably suppose that the degree in which the aggregate values in the Table, both westerly and easterly, may be diminished thereby at the different hours, may be in some measure indicated by the disparity, or the reverse, in the amount of the aggregate values of disturbance in the opposite directions at those hours. Thus we may suppose that at a particular hour, 11 P.M. for example, when the amount of westerly deflections is very small, and of easterly very great, the diminution of the aggregate values of either by mutual counterbalance may be extremely small, while of equal absolute amount in both. Now a very small amount deducted from the large aggregate easterly value will scarcely have any effect whatsoever on the ratio at that hour to its unit or mean hourly value; whereas the same small amount deducted from the far less aggregate westerly value at the same hour would have a far more sensible effect upon its ratio. Assuming, therefore, the probability that westerly and easterly disturbing influences do sometimes coexist and neutralize each other in the record, and that we may in some degree judge of the respective amounts of the conflicting influences at the several

hours by the means above stated, we should be prepared to expect that the ratios which are below unity do not represent the actual variations of the disturbing influences at those hours quite so purely as do the ratios which are above unity; and that they are liable to be affected, though in a very subordinate degree, by the abstraction of the neutralized portion, when the aggregate values which they represent are very small.

Without, however, resting undue weight upon this suggestion, we may safely say that the hours, when the ratios are below unity, are hours of comparative tranquillity, and that their variations from hour to hour are of a far less marked character than during the hours when the ratios exceed unity. Thus viewed, the character of the disturbance-diurnal variations may be conceived to have some analogy with that of the phenomena of the regular solar-diurnal variation. We may imagine the disturbance-variation (either the westerly or the easterly, it is indifferent which is taken),—divided as it is into two portions, by the ratios being in the one case above, and in the other below unity,—to correspond in one of its divisions to the hours when the sun is above the horizon, in the part of the hemisphere where the disturbance may be imagined to originate, whilst the other division, or that in which the ratios are below unity, and manifest hours of comparative tranquillity, may be viewed as the hours of night at the same locality. The solar hours at a station of observation which are characterized by disturbance ratios above unity, will in such case correspond in absolute time with the hours of the *day* at the supposed originating locality, modified (it may be) by a more or less rapid transmission of the disturbance. It will be understood, that in this hypothetical suggestion, the purpose in view is to aid the imagination, if it may be so, in apprehending the *ensemble* of the phenomena as far as they are yet known to us, rather than to advance a theoretical explanation, when we have not yet sufficient facts before us by which it may be judged; it may be remarked, however, that the conception of a double locality of origination of the disturbances (easterly and westerly) in the one hemisphere will present no especial difficulty to those who are conversant with the general facts of terrestrial magnetism.

If our attention be limited to the consideration of the facts observed at a single station, unaccompanied by a view of corresponding phenomena elsewhere, we might be in danger of regarding some of the features, particularly perhaps those which are not the most prominent, as having an accidental rather than a systematic origin; and we might thus lose a portion of the instruction which they may otherwise convey. On this account it has appeared desirable to exhibit the phenomena as observed at a second station, in comparison with those at Kew; and I have selected for this purpose the results of a similar investigation to the present at Hobarton in Tasmania; not only because the facts have been remarkably well determined there, but also because, though it is a very distant station, differing widely in geographical latitude and longitude, and situated indeed in a different hemisphere, there is a striking resemblance

in the laws of the magnetic storms experienced at both. This resemblance, which is not only general, but extends to very minute particulars, is such that it seems impossible to resist the impression that the accordance cannot be accidental; and that the methods of observation and of analysis which have been pursued, have proved themselves well adapted to open to us the knowledge of the existence of systematic laws, pervading and regulating the action of the forces which are in daily operation around us, and are *at least* co-extensive with the limits of our globe; and thus to lead us ultimately to the correct theory of these forces. I have placed therefore beside each other in the next Table the Ratios of Disturbance at the different hours of local solar time at each of the two stations, separating them as before into westerly and easterly deflections, and placing the westerly deflections at Kew in immediate juxtaposition with the easterly at Hobarton, and *vice versâ*, as that obviously constitutes the just comparison. The Hobarton Ratios exhibit the relative prevalence of disturbance at the several hours, derived from hourly observations continued for seven years and nine months, viz. from January 1, 1841 to September 30, 1848; a series unparalleled in duration at any other of the Colonial Observatories, and which has borne admirably, as I shall hope to have a future opportunity of explaining to the Society, an unquestionable test of its substantial accuracy and fidelity. The number of recorded hourly observations was 56,202, of which 7638 differed from their respective normals of the same month and hour by an amount equalling or exceeding 2′·13 of arc, and constituted the body of separated observations from which the aggregate values of disturbance at the different hours and their ratios have been obtained. The proportion of disturbed observations thus separated, to the whole body of observations, is about 1 in 7·35; differing very little from the proportion already noticed as obtained at Kew by a separating value of 3′·3. The disturbing effects due to magnetic storms are therefore somewhat greater at Kew than at Hobarton, though some portion of the difference may be ascribed to the circumstance, that the terrestrial horizontal force, antagonistic to the disturbing forces and tending to retain the magnet in its mean position, is less at Kew than at Hobarton, in the proportion, approximately, of 3·7 to 4·5.

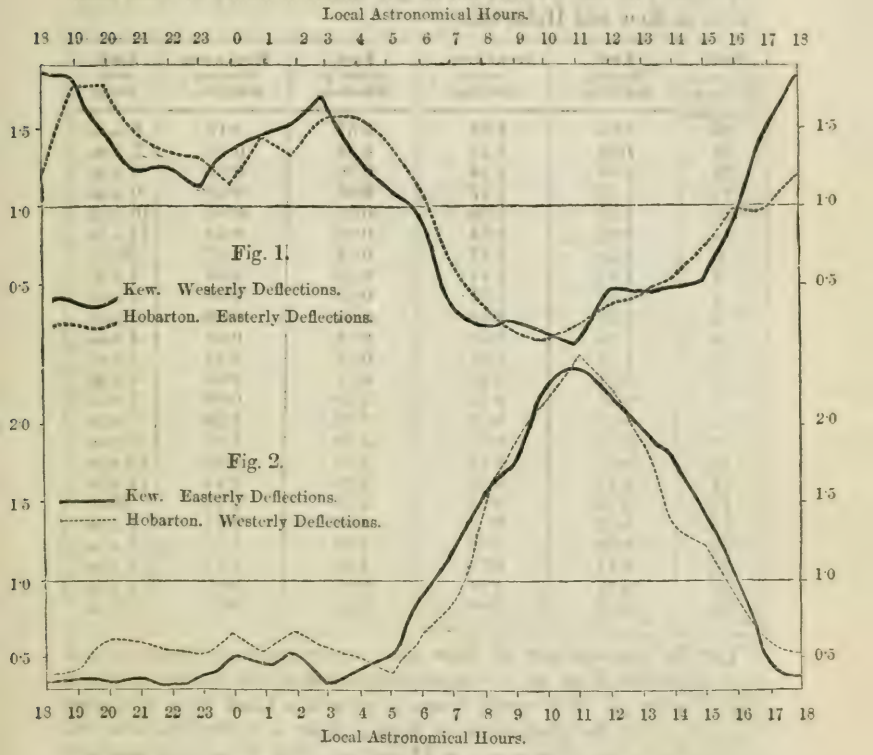
TABLE II.—Showing the comparison of the Ratios of the larger Disturbances of the Declination at the different hours of local solar time at Kew and Hobarton.

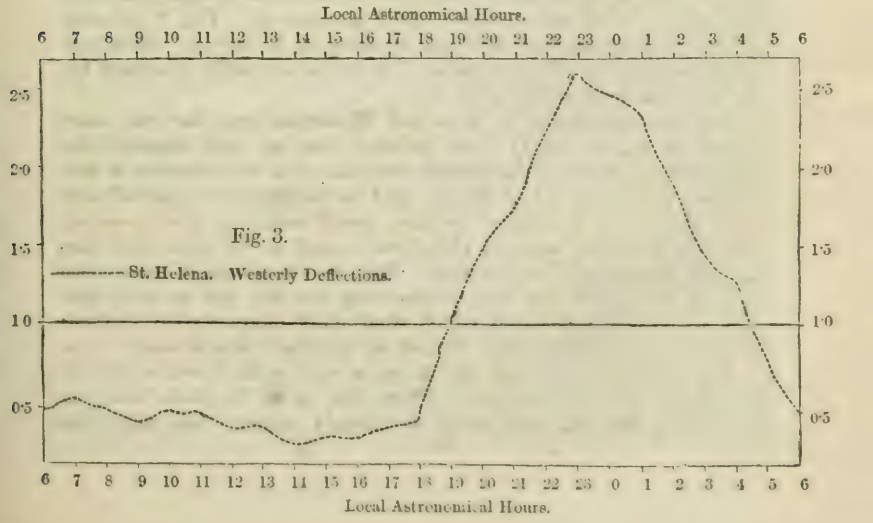
Local astronomical hours.	KEW. Westerly deflection.	HOBARTON. Easterly deflection.	KEW. Easterly deflection.	HOBARTON. Westerly deflection.	Local civil hours.
18	1·85	1·18	0·37	0·42	6 A.M.
19	1·83	1·75	0·38	0·44	7 A.M.
20	1·48	1·76	0·36	0·62	8 A.M.
21	1·23	1·47	0·38	0·60	9 A.M.
22	1·26	1·38	0·33	0·54	10 A.M.
23	1·21	1·31	0·39	0·53	11 A.M.
0	1·38	1·17	0·54	0·67	Noon.
1	1·44	1·44	0·48	0·56	1 P.M.
2	1·53	1·31	0·51	0·68	2 P.M.
3	1·71	1·56	0·34	0·60	3 P.M.
4	1·35	1·58	0·44	0·50	4 P.M.
5	1·15	1·41	0·51	0·42	5 P.M.
6	0·94	1·10	0·91	0·68	6 P.M.
7	0·37	0·62	1·19	0·90	7 P.M.
8	0·22	0·37	1·56	1·50	8 P.M.
9	0·29	0·22	1·79	1·87	9 P.M.
10	0·20	0·17	2·25	2·20	10 P.M.
11	0·12	0·22	2·38	2·43	11 P.M.
12	0·49	0·38	2·21	2·15	Mid.
13	0·47	0·41	1·98	1·74	1 A.M.
14	0·49	0·53	1·80	1·35	2 A.M.
15	0·51	0·71	1·45	1·25	3 A.M.
16	0·97	1·01	0·95	0·85	4 A.M.
17	1·53	0·96	0·45	0·48	5 A.M.

For the convenience of those who prefer graphical illustration, I have represented on an accompanying woodcut the results to which I have referred. The curves drawn in unbroken black lines, in figures 1 and 2, show the phenomena at Kew; those in dotted lines in the same figures, the phenomena at Hobarton. Fig. 1 presents westerly disturbances at Kew, and easterly at Hobarton in comparison with each other; they are obviously allied phenomena. Fig. 2 presents easterly disturbances at Kew and westerly at Hobarton; these are also, obviously, allied phenomena, but are as obviously governed by distinct laws from those in fig. 1.

Had the phenomena at Kew and Hobarton been the only ones known to us, we might have inferred that we had obtained the characteristic forms of the diurnal variations due to the action of two distinct and independent forces; and we might have expected with some degree of confidence to have found curves of corresponding form by a similar analysis elsewhere;—and so far experience has been in accord with expectation. But, as the *forms* of these two pair of curves are not only respectively similar, but as they also correspond in the *hours* at which their chief characteristic features occur, we might also have formed an inference which would have proved erroneous, viz. that the hours as well as the forms would be the same at other stations. Now this is so far from being in accordance with the facts which we already possess, that whilst the

Mean Diurnal Disturbance Variation of the Magnetic Declination.
Figs. 1 and 2, Kew and Hobarton. Fig. 3, St. Helena.





forms present generally a marked resemblance, the hours at different stations exhibit every variety. To exemplify this I have given in a third figure the curve of the westerly disturbance-diurnal variation at St. Helena, of which the form is manifestly the same as that of the two curves in fig. 2, whilst the hours of its most marked features exhibit a difference of nearly 12 hours of local time from those in fig. 2.

It may not be unsuitable on the present occasion to take a brief retrospective view of the progress of our knowledge respecting these remarkable phenomena, videlicet, the *casual magnetic disturbances*, or *magnetic storms*. Antecedently to the formation of the German Magnetic Association and the publication of its first Annual Report in 1837, our information concerning them went no further than that there occurred at times, apparently not of regular recurrence, extraordinary agitations or perturbations of the magnetic needle, which had been noticed in several instances to have taken place *contemporaneously* in parts of the European continent distant from each other; and to have been accompanied by remarkable displays of Aurora, seen either at the locality itself where the needle was disturbed, or observed contemporaneously elsewhere. The opinion which appears to have generally prevailed at this time, was that the Aurora and the magnetic disturbances were kindred phenomena, originating probably in atmospherical derangements, or connected at least in some way with disturbances of the atmospherical equilibrium. They were classed accordingly as "Meteorological Phenomena," and were supposed to have a local, though it might be in some instances a wide, extension and prevalence.

The special purpose of the German Magnetic Association was to subject the "*irregular magnetic disturbances*" (as they were then called in contradistinction to the regular periodical and secular variations) to a more close examination, by means of systematized observations made simultaneously in many parts of Germany. With this view, six concerted days in each year were set apart in which the direction of the declination-magnet should be observed with great accuracy, by methods then for the first time introduced, at successive intervals of five minutes for twenty-four consecutive hours; the meteorological instruments being observed at the same time. The clocks at all the stations were set to Göttingen mean time (Göttingen being the birth-place of the Association), and the observations were thus rendered strictly simultaneous throughout. The high respect entertained for the eminent persons with whom the scheme of the Association originated, obtained for it a very extensive cooperation, not limited to Germany alone, but extending over a great part of the European Continent. The observations of the "Term-days," as they were called, were maintained until 1841, and were all transmitted to Göttingen for coordination and comparison.

The principal results of this great and admirably conducted cooperative undertaking were published in works well known to magneticians. They may be summed up as follows:—The phenomena which were the subjects of investigation were shown to be of casual

and not regular occurrence; to prevail contemporaneously everywhere within the limits comprehended by the observations; and to exhibit a correspondence surprisingly great, not only in the larger, but even in almost all the smaller oscillations; so that, in the words of the Reporters, MM. Gauss and Weber, "nothing in fact remained which could justly be ascribed to *local* causes."

Equally decided were the conclusions drawn against the previously imagined connexion between the magnetic disturbances and derangements of the atmosphere, or particular states of the weather. No perceptible influence whatsoever on the needle appeared to be produced either by wind-storms or by thunder-storms, even when close at hand.

The correspondence in the simultaneous movements of the declination-magnet, so strikingly manifested over an area of such wide extent, was however more remarkable in respect to the *direction* of a perturbation than to its *amount*. The disturbances at different stations, and even, as was expressly stated, at *all* the stations, coincided, even in the smaller instances, in time and in direction, but *with dissimilar proportions of magnitude*. Thus it was found generally that by far the greater number of the anomalous indications were smaller at the southern stations and larger at the northern; the difference being greater than would be due to the difference in the antagonistic retaining force (*i. e.* the horizontal force of the earth's magnetism, which is greater at the southern than at the northern stations). The generality of this occurrence led to the unavoidable inference, that, in Europe, the energy of the disturbing force must be regarded weaker as we follow its action towards the south.

A close and minute comparison of the simultaneous movements at stations in near proximity to each other led to the further conclusion, also stated to be unavoidable, that "various forces must be admitted to be contemporaneously in action, being probably quite independent of each other, and having very different sources; the effects of these various forces being intermixed in very dissimilar proportions at various places of observation according to the directions and distances of these from the sources whence the perturbations proceed." (Resultate aus den Beob. des Mag. Vereins, 1836. pp. 99, 100.) The difficulty of disentangling the complications which thus occur at every individual station was fully foreseen and recognized; and the Report, which bears the initial of M. Gauss, concludes with the remark that "it will be a triumph of science, if at some future time we should succeed in reducing into order the manifold intricacies of the combinations, in separating from each other the several forces of which they are the compound results, and in assigning the source and measure of each."

Such was the state of the inquiry when it was entered upon by the Royal Society. The Report of the Committee of Physics drawn up (*inter alia*) for the guidance of the Magnetic Observatories established by H.M. Government for a limited period in four of the British Colonies, bears date in 1840. The objects proposed by this Report were a very considerable enlargement upon those of the German Association, as well as an extension of the research to more distant parts of the globe. The German observations had been

limited for the most part to one only of the three elements required in a complete investigation. When the German Association commenced its operations, the Declination was the sole element for which an apparatus had been devised capable of recording its variations with the necessary precision. To meet the deficiency in respect to the horizontal component of the magnetic force, M. Gauss constructed in 1837 his bifilar magnetometer, which was employed at Göttingen and at some few of the German stations, concurrently with the Declinometer, in the term observations of the concluding years of the Association. But an apparatus for the corresponding observation of the vertical portion of the Force was as yet wholly wanting; without such an apparatus as a companion to the bifilar, no determination could be made of the perturbations or momentary changes of the magnetic Dip and Force: and without a knowledge of these no satisfactory conclusion in regard to the real nature, amount and direction of the perturbing forces could be expected. The ingenuity of Dr. Lloyd supplied the desideratum by devising the vertical force magnetometer, which, with adequate care, has been found scarcely, if at all, inferior to the bifilar in the performance of its work. The scheme of the British Observatories was thus enabled to comprehend all the data required for the investigation of the casual disturbances, whether that investigation was to be pursued as before by concerted simultaneous observations at different stations, or, as suggested in the Report, *by the determination of the laws, relations and dependencies of the disturbances at individual stations obtained independently and without concert with other observers or other stations.* Thus, in reference to these particular phenomena, the British system was both an enlargement and an extension of the objects of the German Association; but it also embraced within its scope the determinations with a precision, not previously attempted, of the *absolute values* of the three elements, and of *the periodical and progressive changes* to which they are subject; premising however, and insisting with a sagacity which has been fully justified by subsequent experience, on the necessity of eliminating in the first instance the effects of the casual and transitory variations, as an indispensable preliminary to a correct knowledge and analysis of the progressive and periodical changes. A further prominency was given to investigations into the particular class of phenomena which form the subject of this paper, by the declaration that "the theory of the transitory changes is in itself one of the most interesting and important points to which the attention of magnetic inquirers can be turned, as they are no doubt intimately connected with the general causes of terrestrial magnetism, and will probably lead us to a much more perfect knowledge of these causes than we now possess."

The instructions contained in the Royal Society's Report for the adjustments and manipulation of the several instruments provided for these purposes were clear, simple and precise. In looking back upon them after the completion of the services for which they were designed, it is impossible to speak of the instructions otherwise than with unqualified praise. But the guidance afforded by the instructions terminated with the completion of the observations. To have

attempted to prescribe the methods by which conclusions, the nature of which could not be anticipated, should be sought out from observations not yet made, would have been obviously premature. Yet without some discussion of the results, the mere publication of unreduced observations is comparatively valueless. It has been well remarked by an eminent authority, whose opinions expressed in the Royal Society's Report have been frequently referred to in the course of this paper, that "a man may as well keep a register of his dreams, as of the weather, or *any other set of daily phenomena*, if the spirit of grouping, combining, and eliciting results be absent." It was indispensable that the attempt should be made to gather in at least the first fruits of an undertaking on which a considerable amount of public money and of individual labour had been expended; and the duty of making the attempt might naturally be considered to rest on the person who had been entrusted with the superintendence of the Government Observatories. The methods and processes adopted for reducing, combining, eliminating, and otherwise eliciting results were necessarily of a novel description; they were in fact an endeavour to find a way by untrodden paths to simple and general phenomenal laws where no definite knowledge of the origin or mode of causation of the phenomena previously existed. Happily it is not necessary to trespass on the time or attention of the Society by a description of the methods and processes which have been employed to elucidate some of the leading features of the magnetic storms, as these are fully described in the discussions prefixed to the ten large volumes in which the observations at the Colonial Observatories have been printed. It will be only necessary to advert, and that very briefly, to some of the principal conclusions which may be supposed to throw most light on the theory of these phenomena.

The results of the extension of the term-day comparisons to the American Continent, and to the Southern Hemisphere and the Tropics, may first be disposed of in a very few words. The contemporaneous character of the disturbances, which had been shown by the German term-observations to extend over the larger portion of the European Continent, manifested itself also in the comparisons of the term-days in 1840, 1841, and 1842 at Prague and Breslau in Europe, and Toronto and Philadelphia in America, published in 1845; and the same conclusion was obtained by comparing with each other the term-days at the Colonial Observatories, situated in parts of the globe most distant from one another. The days of disturbance still appeared to be of casual occurrence, but were now recognized as affections common to the *whole globe*, showing themselves simultaneously at stations most widely removed from each other. When distant stations were compared, as for example stations in Europe with those in America, and either or both with Tasmania, discrepancies in the amount of particular perturbations, similar to those which had been found in comparing the European stations with each other, presented themselves, but larger and more frequent, and extending occasionally even to the *reversal* of the *direction* of the simultaneous disturbance. Instances were not unfrequent of the same element, or of different elements, being disturbed at the same observation-instant in Europe and

America; and on the other hand, there were perturbations, sometimes of considerable magnitude, on the one continent, of which no trace was visible on the other. Hence it was concluded, with the increased confidence due to this additional and more extensive experience, that various forces proceeding from different sources were contemporaneously in action; and it was further inferred that the most suitable and promising mode of pursuing the investigation was by an endeavour to analyse the effects produced at individual stations, and to resolve them if possible into their respective constituents.

The hourly observations which had been commenced at the Colonial stations in 1841 and 1842, and continued through several subsequent years, furnished suitable materials for this investigation, the first fruits of which were the discovery, that the disturbances, though casual in the times of their occurrence, and most irregular when individual perturbations only were regarded, were, in their *mean* effects, *strictly periodical phenomena*; conforming in each element, and at each station, on a mean of many days, to a law dependent on the solar hour; thus constituting a systematic mean diurnal variation distinct from the regular daily solar-diurnal variation, and admitting of being separated from it by proper processes of reduction. This conformity of the disturbances to a law depending on the solar hours was the first known circumstance which pointed to the sun as their primary cause, whilst at the same time a difference in the *mode* of causation of the regular- and of the disturbance-diurnal variations seemed to be indicated by the fact, that in the disturbance-variation the local hours of maximum and minimum were found to vary (apparently without limit) in different meridians, in contrast to the general uniformity of those hours in the previously and more generally recognized regular solar-diurnal variation.

This first reference of the magnetic storms to the sun as their primary cause, was soon followed by a far more striking presumptive evidence of the same, by a further discovery of the existence of a periodical variation in the frequency of occurrence, and amount of aggregate effects, of the magnetic storms, corresponding in period, and coincident in epochs of maximum and minimum, with the decennial variation in the frequency and amount of the spots on the sun's disk, derived by Schwabe from his own systematic observations commenced in 1826 and continued thenceforward. The decennial variation of the magnetic storms is based on the observations of the four widely distributed Colonial Observatories, and is concurred in by all. This remarkable correspondence between the magnetic storms and physical changes in the sun's photosphere, of such enormous magnitude as to be visible from the earth even by the unassisted eye, must be held to terminate altogether any hypothesis which would assign to the cause of the magnetic disturbances a local origin on the surface or in the atmosphere of our globe, or even in the terrestrial magnetism itself, and to refer them, as cosmical phenomena, to direct solar influence; leaving for future solution the question of the *mode* in which that influence produces the effects which we believe we have thus traced to their source in the central body of our system*.

* The existence of a decennial period of the magnetic storms was not, as some

We may regard as a step towards this solution the separation of the disturbances of the declination into two distinct forces acting in different directions and proceeding apparently from different foci; the phenomena of distinct (though in so many respects closely allied) variations exhibit the same peculiar features at all the stations to which the analysis has hitherto extended, and have been exemplified by the observations at Kew, as shown in the early part of this paper. A similar separation into two independent affections, each having its own distinct phenomenal laws, has followed from an analysis of the same description applied to the disturbances of the magnetic dip and force at the Colonial stations; thus placing in evidence, and tracing the approximate laws of the effects of six distinct forces (two in each element) contemporaneously in action in all parts of the globe, and pointing in no doubtful manner to the existence of two terrestrial foci or sources in each hemisphere from which the action of the forces emanating from the sun and communicated to the earth may be conceived to proceed. Such an ascription naturally suggests to those conversant with the facts of terrestrial magnetism the possibility that Halley's two terrestrial magnetic foci in each hemisphere may be either themselves the localities in question, or may be in some way intimately connected with them. The important observations which we owe to the zeal and devotion of Captain Maguire, R.N. and the Officers of H.M.S. 'Plover,' have made us acquainted with Point Barrow as a locality where the magnetic disturbances prevail with an energy far beyond ordinary experience, indicating the proximity of that station to the source or sources from which the action of the forces may proceed. Now Point Barrow is situated in a nearly intermediate position between what we believe to be the present localities of Halley's northern foci, and at no great distance from either: in such a situation the exposure to disturbing influences proceeding from both might well be supposed to be very great. The displays of Aurora at Point Barrow exceed also in numerical frequency any record received from any other part of the globe.

The further prosecution of this investigation appears to stand in need of some more systematic proceeding than would be supplied by the uncombined efforts of individual zeal. Observations similar to those of the Kew Observatory, made at a few stations in the middle latitudes of the hemisphere, distributed with some approach to symmetry in their longitudinal distances apart, would probably fur-

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have supposed, a fortuitous discovery; but a consequence of a process of examination early adopted and expressly devised, by the employment of a *constant* separating value, to make known any period of longer or shorter duration which might fall within the limits comprised by the observations. The period being decennial, and the epoch of minimum occurring at the end of 1843 or beginning of 1844, the epoch of maximum was necessarily waited for in order to ascertain the precise duration of the cycle. The maximum took place in 1848–1849, the observations in 1850 and 1851 showing that the aggregate value of the annual disturbances was again diminishing as it had been in 1842 and 1843. The process of determining the proportion of disturbance in different years is a somewhat laborious one, and requires time: but in March 1852, I was able to announce to the Royal Society the existence of a decennial variation, based on the concurrent testimony of the observations at Toronto and Hobarton; deeming it proper that so remarkable a fact should not be publicly stated until it had been thoroughly assured by independent observations at two very distant parts of the globe.

nish data, which by their combination might serve to assign the localities from whence the disturbances are propagated—contribute still further to disentangle the complications of the forces which produce them,—and thus hasten the attainment of that "triumph of science" foreseen and foreshadowed by the great geometrician of the last age. Of such a nature was the scheme contemplated by the Joint Committee of the Royal Society and British Association, and submitted to H.M. Government in the hope of obtaining their aid in the execution of such part of it as fell within British dominion; and of thus "maintaining and perpetuating our national claim to the furtherance and perfecting of this magnificent department of physical inquiry." (Herschel in 'Quarterly Review' September 1840, p. 277.) The scheme was no unreasonable one: probably eight or nine stations in the contour of the hemisphere might suffice; and of these we already possess the observations at Toronto; those at Kew are in progress; and self-recording instruments, similar to those at Kew, are now under verification at Kew preparatory to being employed on the Western or Pacific side of the United States Territory, at a point not far from the previously desired Station of Vancouver Island, for which a substitute is thus provided. This Observatory, as well as one at Key West on the southern coast of the United States, in which self-recording instruments are already at work, will be maintained under the authority and at the expense of the American Government, and both have been placed under the superintendence of the able and indefatigable director of the "Coast Survey," Dr. Alexander Dallas Bache. The Russian Observatory at Pekin, the trustworthy observations of which are already known to the Society, is understood to have recommenced its hourly observations, and stands only in need of an apparatus for the vertical force (which might be readily supplied from this country), to contribute its full complement to the required data. More than half the stations may therefore be regarded as already provided for, and there are other Russian observatories in the desired latitudes and longitudes which might be completed with instruments for a full participation.

It would be wrong to conclude these imperfect notices without recognizing how greatly the researches have been aided in their progress by the united and unfailing countenance and support of the Royal Society and of the British Association. The Kew Observatory owes its existence and maintenance to funds most liberally supplied from year to year by the British Association; and the cost of the self-recording magnetic instruments, of which the first instalment of the results has formed the early part of this paper, was supplied from funds at the disposal of the Council of the Royal Society.

GEOLOGICAL SOCIETY.

[Continued from p. 247.]

June 5, 1861.—Leonard Horner, Esq., President, in the Chair.

The following communications were read:—

1. "On the Occurrence of some large Granite Boulders, at a

great depth, in West Rosewarne Mine, Gwinear, Cornwall." By H. C. Salmon, Esq., F.G.S.

The boulders of granite referred to were found in the 50-fathom level below the adit, the adit being 24 fathoms from the surface. One of the boulders was 4 feet 2 inches, and another 3 feet 10 inches in diameter; there were five other smaller boulders or pebbles also met with in the level. The boulders are in the killas close to the lode, and both the lode and the "country" near the lode are made up of brecciated killas. After quoting the details of somewhat similar phenomena formerly observed at Relistian and Herland Mines, the author treated of the probable origin of the boulders in question; and although lodes are regarded by some as having been formed from below upwards, yet in this case the author thinks that the boulders must have had a common origin with the lode, and have been introduced by a fissure from the surface.

2. "On an erect Sigillaria from the South Joggins, Nova Scotia." By Dr. J. W. Dawson, F.G.S.

This specimen, presenting the external markings of leaf-scars and ribs with more than usual clearness and with some instructive peculiarities, has afforded to the author the type of a new species, *Sigillaria Brownii*. Observations on the probable mode of growth, on the structure, and on the classification of *Sigillariæ*, were also given in this paper, together with a *résumé* of the observations previously published regarding *Sigillaria* by Brongniart, Corda, and others.

3. "On a Carpolite from the Coal-formation of Cape Breton." By Dr. J. W. Dawson, F.G.S.

Numerous *Trigonocarpa* belonging to a new species (*Trigonocarpum Hookeri*) occur in a thin calcareous layer in the coal-measures near Port Hood, Cape Breton. The author thinks it highly probable that though some *Trigonocarpa* may have belonged to Conifers, yet in this case they were the seeds of *Sigillaria*.

4. "On a Reconstructed Bed on the top of the Chalk." By W. Whitaker, Esq., B.A., F.G.S.

At some places near Reading (Maidenhatch Farm, about six miles to the W.; and Tilehurst, two miles to the S.W.), and also near Maidenhead, from 18 to 20 feet of broken chalk overlies the true chalk; and in places is overlain by the bottom-bed of the Reading Beds, and therefore must have been reconstructed before the deposition of the Tertiary strata. For the most part, however, in Berkshire the Woolwich and Reading Beds rest on an undisturbed surface of the Chalk. In Wiltshire also the author has observed similarly reconstructed chalk, probably there also underlying Tertiary beds; and he suggests that possibly the local reconstruction of the Chalk may have been contemporaneous with the formation of the Thanet Sands further to the east.

5. "On some of the Higher Crustacea from the British Coal-measures." By J. W. Salter, Esq., F.G.S.

In this paper were described, (1) a new Macrurous Crustacean, under the name of *Anthrapalæmon Grossarti*, from the slaty band of the black-band ironstone of the coal-measures, Goodhock Hill,

Shotts, Lanarkshire. (2) The Macrurous Crustacean of which an imperfect specimen was figured in Mr. Prestwich's memoir on the Coalbrook Dale Coal-field (plate 41, fig. 9, *Apus dubius*): this is referred to a subgenus (*Palæocarabus*) of the genus *Anthrapalæmon*; and another specimen from Ridgeacre Colliery was referred to. (3) A specimen from the Carboniferous Limestone of Derbyshire. (4) A small Crustacean, from the Mountain-limestone of Fifeshire, figured and described by the author in the 'Transactions of the Royal Society of Edinburgh,' vol. xxii. p. 394, as *Uronectes socialis*, but now regarded by him as belonging to the Macrura.

XLI. *Intelligence and Miscellaneous Articles.*

ANALYSIS OF GYROLITE. BY HENRY HOW, PROFESSOR OF CHEMISTRY, KING'S COLLEGE, WINDSOR, NOVA SCOTIA.

THE mineral gyrolite was first described by Professor Anderson of Glasgow* as a new species from the Isle of Skye; it is stated by Greg and Lettsom† to occur without doubt at two localities in Greenland, and, according to Heddle, at Faröe. The only other notice of it that I am acquainted with is by L. Sæmann‡, who mentions that he examined a specimen, no locality being given, mixed or interlaminated with pectolite, and suggests that this mineral losing its alkali becomes gyrolite, and losing its lime becomes Okenite. No other analysis than the original one of Professor Anderson has, I believe, been published; the following account of its occurrence among the minerals of Nova Scotia shows it in such association as affords a mode of explaining its origin by change in apophyllite:—I met with it in Anapolis County, N. S., some twenty-five miles south-west of Cape Blomidon, between Margaretville and Port George, on the surface of fractured crystalline apophyllite; and on further breaking the mass a good many spherical concretions of pearly lustrous plates were observed in the interior, of sizes varying from that of a pin's head to nearly half an inch in diameter: their outline was well defined, and the external characters as given by Anderson were recognized on examination; it afforded the following results on analysis:—The mineral was ignited for water, and the residue treated with HCl; the resulting dried silica was weighed, and then fused with carbonated alkali; and the weight of the small quantities of alumina, &c. so separated was deducted from that of the first silica. I place my numbers by the side of those of Anderson, and give the calculated per-centages for his formula:—

	How.	Anderson.	Calculation.		
Potassa . . .	1·60	....			
Magnesia . .	0·08	0·18			
Alumina . . .	1·27	1·48			
Lime	29·95	33·24	32·26	2CaO	= 56
Silica	51·90	50·70	52·18	$2SiO^3$	= 90·6
Water . . .	15·05	14·18	15·55	3HO	= 27·0
	99·85	99·78	99·99		173·6

* Trans. Roy. Soc. Edinb., and Phil. Mag. Feb. 1851.

† Manual of Mineralogy, p. 217.

‡ First Supp. to Dana's Mineralogy, p. 9.

A general accordance is observed sufficient to show the identity of chemical composition in the minerals examined; the small quantity of potassa present in my specimen probably modified the blowpipe character a little, as I found it not to exfoliate completely, and it fused without any difficulty, and even with some ebullition.

Some of the numerous cavities in the apophyllite were empty, some entirely filled with gyrolite, and in others separate plates of this mineral were standing edgewise, leaving vacant spaces, while upon and by the side of the plates were in some cases rhombohedral crystals which proved to consist of calcite, and were sometimes present alone in the cavities, which varied from being quite shallow to half an inch in depth. It is mentioned by Anderson that gyrolite occurs associated with stilbite, Laumonite, and other zeolites, and is sometimes found coating crystals of apophyllite.

The difference in chemical composition between apophyllite and gyrolite is very well seen on comparing the respective theoretical per-centages of their constituents, thus:—

	SiO^3.	CaO.	KO.	HO.
Apophyllite .	= 52·70	26·00	4·40	16·70+HF variable.
Gyrolite · ·	= 52·18	32·26		15·50

and the existence of the calcite in the cavities seems clearly to show that the gyrolite is formed from the apophyllite by the waters which deposited the carbonate of lime reacting on the silicate of potash, and dissolving out at the same time the fluorine as fluoride of calcium*: trial was made for fluorine on two fragments of the gyrolite, but no evidence of its existence obtained.—Silliman's *American Journal*, July 1861.

PRODUCTION OF THE GREEN MATTER OF LEAVES UNDER THE INFLUENCE OF THE ELECTRIC LIGHT. BY M. HERVÉ MANGON.

It appeared interesting to ascertain whether the green matter developed so readily in young leaves exposed to the sun, was also produced under the influence of the bright light of the electric lamp. This experiment has been tried by the kind aid of M. Allard, chief engineer of lighthouses, who has allowed me to use for several days the powerful apparatus under his control.

The electricity was produced by a powerful electro-magnetic machine driven by a steam-engine. The light was that of a charcoal lamp.

The lamp was lit for eleven hours on the 30th of July, twelve hours on July 31, Aug. 1, and Aug. 2, and eleven hours and a half on Aug. 3. The temperature of the air varied from 22° to 25° C., and that of the earth from 19° to 21° C.

On the 30th of July, at 8 in the morning, small flower-pots, each containing four grains of rye, sown respectively on the 24th, 26th, 27th, and 28th of July, were placed in a perfectly dark room, about a yard from the lamp, and about 2 feet below the luminous focus, and without the interposition of any glass.

The grains sown on the 24th and 26th had sprouted; the stalks

* See Dana's Mineralogy, vol. i. pp. 232, 233.

were 0·005 metre to 0·012 metre in length. There was a slight green tint on the top of one of these plants; the other was quite white. The grains sown on the 27th and 28th of July had not sprouted on the 31st of July at 2 o'clock; the plants sown on the 24th and 26th of July were 0·010 metre to 0·060 metre in length; they were all *very green*, and strongly turned towards the light. The grain sown on the 27th of July had sprouted; the plants were 0·020 metre to 0·030 metre high, and there was a little green on the top of one of them.

At 1 o'clock on the 1st of August the plants continued to grow just as in the light. The rye sown on the 28th of July had sprouted, but showed no green.

On the 2nd of August, at 2 o'clock, all the plants continued to grow; the rye which had sprouted on the night before was decidedly green.

The seeds kept in the dark for the sake of comparison, gave plants which were completely yellow.—*Comptes Rendus*, Aug. 5, 1861.

NATURE OF THE DEPOSIT WHICH FORMS UPON THE COPPER EMPLOYED IN REINSCH'S TEST FOR ARSENIC.

Lippert has made a careful examination of the crust which forms upon bright metallic copper when this is placed in a solution of arsenic acidified with chlorhydric acid. This coating had been pretty generally mistaken for metallic arsenic until Fresenius (in his *Anleitung zur qualitativen Analyse*, 10te Aufl., Braunschweig, 1860, p. 141) called attention to the fact that it contained a large quantity of copper. From the experiments of Lippert, it now appears that the crust in question contains only 32 per cent. of arsenic, 68 per cent. of its weight being copper. This composition having been nearly constant in several specimens which he analysed, Lippert maintains that the compound is a definite alloy, $As Cu^5$. When ignited, at the temperature of a combustion furnace, in a current of hydrogen, the compound lost only 7 per cent. of its weight, an alloy of the composition $As Cu^6$ (same as that of the mineral Domeykite of F. Field) being formed.

The delicacy of Reinsch's test is evidently directly referable to the large amount of copper which the characteristic coating contains; for a proportionally small quantity of arsenic is thus obtained in an enlarged and, as it were, more tangible form. But, on the other hand, it is not easy to prove in a simple manner the presence of arsenic in this crust; for only a small portion of the arsenic can be volatilized in a current of hydrogen, and even if the alloy be first oxidized in a current of air and then reduced in a current of hydrogen, the per-centage of arsenic only falls from 32 to 20. By far the largest portion of the arsenic is therefore kept out of sight.

For the details of this interesting research, and the author's discussion of the proposition of Reinsch and v. Kobell to estimate arsenic quantitatively by determining the amount of copper which dissolves while the arsenic is being precipitated, we must refer to the original article.—*Journ. für Prakt. Chemie*, vol. lxxxi. p. 168.

THE

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PHILOSOPHICAL MAGAZINE

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[FOURTH SERIES.]

NOVEMBER 1861.

XLII. *Chemical Analysis by Spectrum-observations.—Second Memoir.* *By* G. KIRCHHOFF *and* R. BUNSEN*.

[With a Plate.]

IN our first memoir on this subject, a translation of which appeared at page 89 of the 20th volume of this Magazine, we showed that the bright lines observed in the spectra of the incandescent vapour of certain metallic compounds may be employed as the most sure and delicate tests for ascertaining the presence of these metals. The analytical method founded upon such observations is of special importance when applied to the examination of groups of substances either occurring in very small quantities or possessing nearly identical chemical characters, because in these special cases this mode of examination introduces a whole series of most delicate distinctive reactions, hitherto wholly unknown. Considering how much more delicate the spectrum reactions are than the ordinary chemical tests, it appeared to us that this method would serve especially well for discovering the presence of substances which might have been overlooked by the rough methods previously employed, either because the bodies occurred in very small quantities, or because they were not distinguishable by the ordinary tests from other well-known bodies. This assumption was verified by the first appeal to experiment; for we have succeeded in discovering the presence of two new alkaline metals in addition to potassium, sodium, and lithium, notwithstanding that they both give all the characteristic precipitates of the potash salts, and occur in such minute quantities that, in order to obtain a few grammes of these substances, or a sufficient amount for investigation, we had to operate upon 44,000 kilogrammes (about 40 tons) of the mineral water of Dürkheim, and upon 180 kilogs. of Lepidolite.

* From Poggendorff's *Annalen*, No. 7, 1861. Communicated by Professor Roscoe.

If a drop of the mother-liquor of the Dürkheim water be brought into the flame of the spectrum-apparatus, the characteristic lines of sodium, potassium, lithium, calcium, and strontium are at once seen. If the lime, strontia, and magnesia be separated according to well-known processes, and if the residual alkaline bases in the form of nitrates be washed out with alcohol and the lithium removed as completely as possible by precipitation with carbonate of ammonium, a mother-liquor is obtained which in the spectrum-apparatus shows the lines of sodium, potassium, and lithium, but besides these, two splendid blue lines situated close together, and almost coinciding with the blue strontium line Sr δ.

As no known elementary body produces two blue lines in this portion of the spectrum, we may consider the existence of this hitherto unknown alkaline element as thus placed beyond a doubt.

The facility with which a few thousandths of a milligramme of this body may be recognized by the bright blue light of its incandescent vapour, even when mixed with large quantities of the more common alkalies, has induced us to propose for it the name *Cæsium* (and the symbol Cs), derived from the Latin "cæsius," used to designate the blue of the clear sky*.

If Saxony lepidolite be treated by any of the known plans for separating the alkalies from the other constituents, and if the solution of the alkalies thus obtained be precipitated with bichloride of platinum, an abundant precipitate is formed, which, when examined in the spectrum-apparatus, shows only the bright potassium lines. If this precipitate be repeatedly washed with boiling water, and the residual salt occasionally examined in the apparatus, two splendid violet lines, lying between the strontium line Sr δ and the blue potassium line Kβ, will be noticed on the gradually fading continuous background of the potassium spectrum. These new lines increase in brilliancy as the washing is continued, and a number more appear in the red, yellow, and green portions of the spectrum.

None of these lines belong to any previously known body. Amongst them are two which are especially remarkable, as lying beyond Fraunhofer's line A and the potassium line Kα coincident with it, and therefore situated in the outermost portion of the red solar rays. Hence we propose for this new metal the name *Rubidium* (and the symbol Rb), from the Latin "rubidus," which was used to express the darkest red colour†.

Before describing the special spectra of cæsium and rubidium,

* Aulus Gellius, in the *Noctes Atticæ*, ii. 26, quotes Nigidius Figulus as follows:—Nostris autem veteribus cæsia dicta est, quæ a Græcis γλαυκῶπις, ut Nigidius ait, de colore cœli quasi cœlia.

† Aulus Gellius, *Noctes Atticæ*, ii. 26. Rubidus autem est rufus atrior et nigrore multo inustus.

Fig. 4.

Fig. 1

Fig. 2

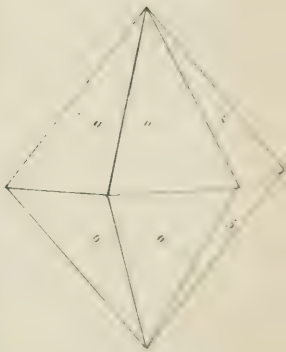


Fig. 3

we proceed to recount the experiments which one of us has conducted for the purpose of establishing the properties of the two new elements, and their more important compounds.

I. *Of the Preparation, Atomic Weight, and occurrence of the Rubidium Compounds.*

The pure chloride of rubidium was procured from the saline residue obtained by fusing a mass of about 150 kilogrammes of Saxony lepidolite, from which the alkaline earths and lithium salts had been removed. The separation of the new element, and the preliminary determination of its atomic weight, were effected as follows:—

The saline residue was dissolved in water, and treated with about 100 grms. of bichloride of platinum, a quantity, however, quite insufficient to precipitate all the potassium; the double platinum salt was then boiled out twenty times with a small volume of water, and the boilings added to the original solution of the saline residue, whereby a precipitate again occurred, which was treated exactly as the former. In the course of the process of continued boiling with small quantities of water, the solution, which originally was of a dark yellowish-brown colour, becomes gradually lighter, so that it is easy to see, by the light colour of the precipitate remaining unchanged, the point at which the boiling-out has been continued long enough. The extraction is carried on until the whole of the precipitate formed by the saline residue dissolves on repeated boiling with small quantities of water. The several platinum precipitates, after having been again purified by treating them altogether with boiling water, are dried and reduced in a current of hydrogen gas, by which means a mixture of metallic platinum and impure chloride of rubidium is obtained, the latter being extracted by water. This aqueous solution is diluted, and, whilst boiling, again precipitated by chloride of platinum, and the insoluble double salt reduced, as before, in a current of hydrogen.

Of the chloride of rubidium thus prepared, which we will designate as portion A, 2·2496 grammes gave on precipitation 2·7688 grms. chloride of silver. A portion of this same preparation A was dissolved in about thirty times its weight of water, and precipitated whilst hot with a solution of chloride of platinum so diluted that the precipitate appeared only after the lapse of a few minutes. As the liquid cooled, the precipitate became more dense; and when the temperature had sunk to about 40° C., it was filtered off, dried, and reduced in hydrogen as described. The chloride thus prepared we will call portion B; of this preparation 0·9022 grm. gave 1·0712 grm. chloride of silver. A similar mode of separation was adopted in the case of the salt B, and another salt, which we call portion C, obtained; 1·3540 grm. of

this portion yielded 1·6076 grm. chloride of silver. By a repetition of this process on the salt C, a product D was prepared, of which 1·9486 grm. gave 2·3091 grms. chloride of silver. The quantities of chloride of silver obtained from one part by weight of chloride of rubidium after each of these purifications, are therefore,—

A	1·2308
B	1·1873
C	1·1873
D	1·1850

These numbers prove that the products of the three last preparations possess a constant composition. The bright spectrum-lines of cæsium and lithium were almost invisible in the last of these preparations; and the line Kα of potassium could not be seen at all in our spectrum-apparatus; so that we may fairly conclude that the product of the last preparation is pure chloride of rubidium.

In order to obtain a still further proof of the purity of the chloride thus prepared, a method was employed of which we shall again have to speak when discussing the mode of separation of cæsium from potassium and rubidium. This consists in treating the three caustic alkalies with carbonic acid until about one-fifth by weight of the whole mass is converted into carbonates, and then extracting the anhydrous salt with alcohol. If an alkali possessing greater or less basic properties than rubidium, and having a different atomic weight, were present together with this metal, the alcoholic solution must possess a composition differing from that of the residue; the portion of caustic alkali which dissolved in alcohol yielded, however, a chloride possessing a composition identical with that obtained from the portion of alkali undissolved by the alcohol, 0·5116 grm. of the former yielding 0·6078 grm. chloride of silver, or 1 part of chloride giving 1·1830 of chloride of silver, closely corresponding with the previous results. If we only consider the precipitation of the products possessing constant composition, and if we take, according to Stas, the atomic weight of silver to be 107·94, and that of chlorine 35·46, we obtain the following numbers for the atomic weight of rubidium on the hydrogen scale:—

B	85·31
C	85·32
D	85·55
E	85·24

or a mean of Rb = 85·36. The weight of the atom of the new metal is therefore more than twice as great as that of potassium. Although the numbers thus obtained do not coincide with the degree of accuracy which may be desirable in determinations of atomic weights, we believe that the mean experimental number

does not differ from the true combining proportion more than is the case with a large proportion of the atomic weights at present considered as correct, and received without question.

It is almost needless to add that the residues obtained by the treatment above described, when operated upon again, yielded a considerable quantity of chloride of rubidium.

Although impossible to determine with exactitude the quantity of rubidium contained in lepidolite, it appeared of interest to ascertain this as accurately as possible. For this purpose a specimen of lepidolite was employed, found at Rozena near Hradisko, which was seen by means of spectrum-analysis to contain traces of cæsium as well as rubidium. The solution obtained from 13·509 grms. of this lepidolite fused with lime, was precipitated in the usual manner with chloride of platinum, and the precipitated double chlorides of potassium, rubidium and platinum reduced with hydrogen, yielding 2·0963 grms. of the chlorides of potassium and rubidium. These salts were again precipitated by chloride of platinum, and the precipitate thus obtained, boiled out with *small* quantities of water until the solution appeared of a light yellow colour. The united wash waters, on evaporation and cooling, deposited a second crop of crystals, which were treated in a similar way to the first precipitate. The platinum double salt which separated out a third time, was likewise submitted to the same treatment, and the operation repeated until the precipitate formed, on concentrating the wash-waters, easily dissolved on boiling in a small quantity of water without leaving any residue. The whole of the insoluble platinum double salt yielded, after reduction in a current of hydrogen, 0·0421 grm. of chloride of rubidium, corresponding to 0·24 part of oxide of rubidium in 100 parts of the lepidolite in question. This determination, together with an analysis of the other constituents, made by Mr. Cooper in my laboratory, gives the following composition for the lepidolite from Rozena:—

Silicic acid	50·32
Alumina	28·54
Peroxide of iron	0·73
Lime	1·01
Magnesia	0·51
Oxide of rubidium	0·24
Oxide of cæsium	trace.
Lithia	0·70
Fluoride of lithium	0·99
Fluoride of sodium	1·77
Fluoride of calcium	12·06
Water	3·12
	99·99

We have assured ourselves in a series of spectrum-analytical investigations, which we here omit, as we shall return to the subject when discussing the properties of the cæsium compounds, that in almost all mineral springs containing chloride of sodium, traces of rubidium compounds accompany the salts of potassium and sodium; so that, although rubidium is found in but small quantities, it is by no means a body of rare occurrence.

II. *Of Metallic Rubidium and some of its compounds.*

a. *The Metal.*

As the total quantity of pure material which we possessed scarcely exceeded one ounce, it would have been unwise to waste the whole of this in one experiment upon the reduction of the metal from the carbonate, and we therefore for the present confined ourselves to separating the metal by means of electrolysis. If a current be passed through fused chloride of rubidium, the positive pole consisting of a rod of graphite, and the negative pole being formed of an iron wire, the metallic rubidium, is seen to rise to the surface of the liquid from the latter, and burns with reddish colour on coming in contact with the air. When the iron wire is surrounded by a small glass bell through which a current of pure dry hydrogen is led, the metal does not burn, but it does not collect in the hollow bell, as it disappears as soon as it is liberated, uniting with the chloride to form a subchloride, which dissolves in the fused mass. This subchloride imparts a deep blue colour to the salt in the neighbourhood of the iron pole; and although this blue mass is perfectly transparent and does not exhibit, either when examined by the naked eye or with the microscope, any trace of a metallic substance, it decomposes water with evolution of hydrogen and with formation of a colourless solution having a strong alkaline reaction; chloride of potassium also forms, under similar circumstances, a blue subchloride. If the reduction be repeated with a mixture containing an equal number of atoms of chlorides of calcium and rubidium at the temperature, almost below redness, at which this mixture fuses, a mass is obtained which evolves large quantities of hydrogen when thrown into water, and from which small grains of metal are thrown out, which ascending take fire spontaneously on coming into contact with the air. The metal cannot, however, be obtained in this way in sufficient quantity to be properly investigated. The amalgam of rubidium can, on the contrary, be very easily prepared from a concentrated solution of chloride of rubidium when metallic mercury is used as the negative pole, and a platinum wire is employed as the positive pole. The mercury is thus quickly changed into an amalgam of rubidium, which on cooling appears as a solid crystalline brittle mass of a

silver-white colour. This amalgam decomposes water at the ordinary temperature, absorbs oxygen from the air, becoming hot, and being covered with a white coating of caustic hydrated oxide of rubidium. Rubidium amalgam is strongly electro-positive in respect to potassium-amalgam when a circuit is completed with both by means of the chlorides of rubidium and potassium.

Potassium, therefore, can no longer be considered as the most electro-positive element, for in the foregoing experiment it has been shown to be more electro-negative than rubidium.

b. *Hydrated Oxide of Rubidium.*

This substance is best prepared from the sulphate; the latter salt is dissolved in 100 parts of water, and the solution boiled for some time to free it from air; to the boiling solution hydrate of baryta is cautiously added; the sulphate separates quickly out, so that the point of complete precipitation is easily and accurately reached. If the liquid be now quickly evaporated in a silver basin, the hydrated oxide is obtained as a white, or greyish-white, porous mass, which melts and fuses quickly almost below a red heat. It does not lose its water of hydration at a red heat; on cooling, it solidifies to a brittle though not easily breakable mass, which does not exhibit any crystalline structure. This substance is completely and quickly volatilized when placed in a flame; and placed in contact with water, it dissolves with evolution of great heat. Placed on the skin it acts as a powerful caustic, resembling the hydrate of potash. Exposed to the air it rapidly deliquesces, forming a syrupy liquid which possesses the peculiar oily feeling, when placed on the finger, characteristic of the common alkalies; and it gradually absorbs carbonic acid, at first becoming carbonate, and at last forming bicarbonate of rubidium. Alcohol dissolves this substance as easily as it does caustic potash, and a thick oily liquid is produced. As regards alkaline reaction and alkaline taste, it is not surpassed by potash. The alkali cannot be evaporated in platinum vessels, as it attacks this metal as strongly as caustic potash.

0·7200 grm. of this hydrated oxide of rubidium yielded 0·9286 grm. of sulphate. Hence it consists of—

		Calculated.	Found.
RbO . . .	93·36	91·21	90·29
HO . . .	9·00	8·79	9·71
	102·36	100·00	100·00

The somewhat large excess of water here found is explained by the difficulty of obtaining the salt perfectly free from carbonic acid. We have, as yet, not made experiments to determine whether rubidium possesses any higher or lower oxides.

c. *Monocarbonate of Rubidium.*

This salt is best prepared from the sulphate of rubidium by precipitating with baryta water, and evaporating the solution of the caustic alkali to dryness with carbonate of ammonium. The excess of baryta added remains behind on treating the mass with water. The solution yields on concentration indistinct crystals and crystalline crusts of hydrated carbonate of rubidium, which, on heating strongly, melt in their water of crystallization, and leave at last a porous mass, melting at a red heat, and solidifying on cooling to an opake white crystalline salt. The anhydrous salt is strongly hygroscopic, and dissolves in water with evolution of heat. It has a caustic and corrosive action upon the skin. The alkaline reaction of the salt is so powerful that boiled water, to which only $\frac{2}{10,000}$ths of the salt has been added, imparts a distinct alkaline reaction to red litmus paper. The salt is almost insoluble in boiling absolute alcohol, 100 parts of alcohol only dissolving 0·74 of the salt. When fused in a platinum crucible, it does not lose its carbonic acid, even at very high temperatures. 1·4632 grm. of the salt which had been fused for some time, lost 0·2748 grm. of carbonic acid upon treatment with sulphuric acid. Hence the composition of the salt is as follows:—

		Calculated.	Found.
RbO . . .	93·36	80·93	81·22
CO^2 . . .	22·00	19·07	18·78
	115·36	100·00	100·00

d. *Bicarbonate of Rubidium.*

The aqueous solution of the monocarbonate is easily converted into the acid salt when placed in contact with an atmosphere of carbonic acid. If the solution be allowed to evaporate at the ordinary atmospheric temperature over sulphuric acid, the salt forms shining crystals, permanent in the air, possessing a prismatic form, but of which no sample sufficiently well crystallized for exact measurement could be obtained. The crystals give a very slightly alkaline reaction, and they possess a cooling, non-caustic, agreeable taste, similar to that of saltpetre. On heating, they easily lose the second atom of carbonic acid. They are very soluble in water; and the aqueous solution gives off carbonic acid on boiling, probably owing to the formation of a sesquicarbonate.

0·5116 grm. of monocarbonate of rubidium was dissolved in water in a weighed platinum crucible, and left for fourteen days in an atmosphere of carbonic acid, which was slowly from time to time renewed. After the solution had been evaporated at the ordinary temperature over sulphuric acid, the mass was again

moistened with a solution of carbonic acid in water, and again dried in the same way until no further loss of weight occurred. The salt then weighed 0·6878 grm. Hence it is seen that the bicarbonate of rubidium has a composition analogous to bicarbonate of potassium, or is represented by the formula—

		Calculated.	Found.
RbO . . .	93·36	63·79	63·72
$2CO^2$ . . .	44·00	30·06	
HO	9·00	6·15	
	146·36	100·00	

e. *Nitrate of Rubidium.*

This salt crystallizes from aqueous solution, when quickly cooled, in long indistinct crystals. When the crystallization is conducted more slowly, double hexagonal prisms terminated with less distinct double hexagonal pyramids are obtained in a state fit for measurement. The crystals invariably incline to a prismatic form, and belong to the hexagonal system, corresponding to a ratio of the axes of

$$1 : a \text{ as } 1 : 0{\cdot}7097.$$

This ratio belongs to an obtuse hexagonal dodecahedron, having polar angles of 78° 40′, and basal angles of 143° 0′. The pyramidal faces were very imperfectly formed, so that the measurement of the angles could not be very exactly made. The faces P . ∞P . P2 . ∞P2, Plate V. fig. 1, were the only ones observed.

	Found.	Calculated.
$p-p_1$ . . .	149° 49′	150°
p_1-p . . .	149 53	150
$r-p$* . . .	129 20	

The nitrate of potassium crystallizes, as is well known, in the rhombic system, but according to Frankenheim it occurs sometimes as a secondary hexagonal form whose hemihedral form corresponds to a hexagonal dodecahedron having polar angles of 106° 40′. This form corresponds to a hexagonal dodecahedron of another order than that in which nitrate of rubidium crystallizes; to this, however, we will recur in speaking of the nitrate of cæsium.

The nitrate of rubidium is anhydrous, but, like saltpetre, it contains water enclosed in the pores of the crystals, which therefore decrepitate on heating. Near red heat it fuses without decomposition to a clear liquid, and on cooling solidifies to a striated crystalline mass. When heated to a higher point it loses oxygen, and forms nitrite together with caustic oxide of

* This angle served for the calculation of the ratio of the axes.

rubidium, which acts rapidly upon the platinum vessels. Brought into the colourless gas flame on a platinum wire, the salt is completely volatilized. It is much more soluble in water than saltpetre; 100 parts of water at 0° C. dissolve 20·1, and at 10° C. 43·5 parts of the salt. Under the same circumstances water only dissolves 13·3 and 20·4 parts of saltpetre.

2·3543 grms. of the salt, when decomposed by sulphuric acid, yielded 2·1306 grms. of sulphate of rubidium. Hence the salt consists of—

		Calculated.	Found.
RbO . . .	93·36	63·35	63·36
NO^5 . . .	54·00	36·65	36·64
	147·36	100·00	100·00

f. *Sulphate of Rubidium.*

The acid salt having the formula RbO, $2SO^3$ fuses like the corresponding potassium salt near redness, and when more strongly heated froths considerably, losing half its sulphuric acid, and leaving a solid residue, fusible only at a white heat. If the aqueous solution of this neutral salt be slowly evaporated, fine large hard brilliant crystals are obtained, which belong to the rhombic system, and possess a ratio of the axes of $a : b : c$ as 0·5723 : 1 : 0·7522, corresponding to a rhombic octahedron whose basal angles are 113° 6′, and whose polar angles are 121° 6′ and 87° 8′. The crystal represented in fig. 2 gave the following surfaces:—

	P . ∞ . $\breve{P}2$. Found.	Calculated.
$o-o$ . . .	131° 6′	
$o-o_1$ . . .	113 6	
$s-o$ . . .	130 36	130° 42′
$s-s_1$ . . .	112 43	112 46

This salt is therefore isomorphous with sulphate of potassium, which, according to Mitscherlich, possesses the following ratio of the axes:—$a : b : c$ as 0·5727 : 1 : 0·7464. The sulphate of rubidium is anhydrous, perfectly unalterable in the air, and it possesses a peculiar taste, resembling that of sulphate of potassium. On heating, it decrepitates, and loses its tranparency. Placed on a platinum wire in the flame, it is completely volatilized. 100 parts of water at +70° C. dissolve 42·4 parts of this salt; under the same conditions only 9·58 parts of sulphate of potassium are dissolved.

1·0098 grm. of this salt yielded 0·8872 grm. of sulphate of barium. Hence it consists of—

		Calculated.	Found.
RbO . . .	93·36	70·01	69·86
SO^3 . . .	40·00	29·99	30·14
	133·36	100·00	100·00

With sulphate of alumina this salt forms rubidium alum, $RbO\,SO^3 + Al^2O^3\,3SO^3 + 24HO$, which can be obtained very easily in large, bright, transparent crystals belonging to the regular system. Besides the prominent faces 0, the following, both $\infty 0$ and $\infty 0\infty$, are seen to occur. The crystals are unalterable in the air, and in other respects closely resemble those of potash alum.

Sulphate of rubidium also forms with the sulphates of the magnesian class of bases a series of double salts corresponding to the formula $KO\,SO^3 + NiO\,SO^3 + 6HO$, and isomorphous with the respective potassium salts. These double rubidium salts are more difficultly soluble than the sulphate of rubidium itself, and can be easily obtained in large well-developed crystals. They generally exhibit the following faces :—

$$\infty P.0P. + P.P\,\infty. + 2P\,\infty.$$

h. *Chloride of Rubidium.*

This compound crystallizes indistinctly from aqueous solution upon quick evaporation or cooling; but on allowing the solution to evaporate slowly, cubic crystals are obtained. No other combination besides the cubic faces $\infty 0\infty$ were noticed. The crystals are unalterable in the air; they decrepitate on warming, and they fuse when heated to a temperature just below a red heat. Brought into the flame on a platinum wire, the salt volatilizes quickly and completely. 100 parts of water at $+1°$C. dissolve 76·38 parts, and at $+7°$C. 82·89 parts of this salt. Under similar circumstances 29·47 and 31·12 parts of chloride of potassium are dissolved.

0·9740 grm. of this chloride of rubidium yielded 1·1541 grm. of chloride of silver, hence the salt consists of—

		Calculated.	Found.
Rb . . .	85·36	70·65	70·30
Cl . . .	35·46	29·35	29·70
	120·82	100·00	100·00

i. *Double Chloride of Platinum and Rubidium.*

To obtain this compound a solution of a salt of rubidium is precipitated with bichloride of platinum.

The precipitate is of a light yellow colour, immediately depo-

siting on boiling as a fine heavy powder, which, examined under the microscope, is seen to consist of small shining regular octahedrons, transparent, and of a honey-yellow colour. The compound is quite insoluble in alcohol, and much more difficultly soluble in water than the corresponding potassium salt. 100 parts of water dissolve of this salt—

at	0·0 C.	0·193 part
at	13·5 C.	0·135 „
at	48·0 C.	0·195 „
at	60·0 C.	0·263 „
at	100·0 C.	0·641 „

The above numbers are the mean of a number of experiments which agree so well amongst themselves, that the result given at 13°·5 C. as the minimum of solubility may be regarded as certain. This seems to show that at a lower temperature the salt contains water of crystallization.

The bichloride of platinum in the salt loses some of its chlorine when a current of hydrogen is passed over it in the cold; and the whole of this chlorine completely disappears when the salt is heated, leaving a residue of chloride of rubidium and metallic platinum. For the purpose of analysing the salt, 1·9398 grm. of the double salt prepared from pure bichloride of platinum, and completely dried at 150° C., was reduced in a current of hydrogen, and thus lost 0·4850 grm. The chloride of rubidium, obtained by extracting the residue with water, weighed 0·7891 grm., and yielded 0·9252 grm. chloride of silver. The reduced platinum weighed 0·6620 grm. Hence the composition of the salt is found to be—

			Calculated.	Found.
Bichloride of platinum	Pt.	91·10	34·08	34·13
	Cl^2	70·92	24·39	25·00
Chloride of rubidium	Rb	85·36	29·35	28·88
	Cl.	35·46	12·19	11·79
		282·84	100·01	99·80

The somewhat considerable deviation of the found from the calculated numbers is to be accounted for by the fact that the reduction of the salt was made in a crucible with a perforated lid, and that a small quantity of the chloride of rubidium was thus volatilized.

III. *Of the Occurrence, Preparation, and Atomic Weight of the Cæsium Compounds.*

We have always found this metal associated with sodium, potassium, and rubidium. It occurs in the largest quantity in the mother-liquor from the mineral water of Dürkheim; and we

have employed this liquor as the source of the material which we have used in the present investigation.

The following observations served as the foundation for the mode of separating the cæsium-salts from the compounds of the other alkalies.

If the mother-liquors of the Dürkheim mineral water be freed from alkaline earths by the ordinary methods, so that the mass which remains after volatilization of the ammoniacal salts consists solely of salts of the alkalies, and if the solution of these salts be then treated with bichloride of platinum, a yellow crystalline precipitate is obtained, which, when placed in the spectrum-apparatus, exhibits an intense potassium reaction, but shows no trace of the blue lines of cæsium. If the platinum precipitate be boiled out twenty times with small quantities of water, the successive solutions, as is the case with the rubidium compounds, gradually become less and less yellow-coloured. Examined from time to time in the spectrum-apparatus, the potassium lines Kα, Kβ are seen to grow fainter and fainter, and the blue cæsium lines gradually become visible on the fading background of the continuous potassium spectrum. The double chloride of platinum and cæsium is therefore, like the corresponding rubidium-salt, more difficultly soluble than the chloride of platinum and potassium.

In our first experiment, conducted as above described, we employed 50 grms. of the mother-liquor, and obtained only 1·2 milligramme of the impure double chloride of platinum and cæsium; still, with this minute quantity, the spectrum reaction was so well defined and characteristic that we did not hesitate at once to commence the preparation of the salts of the new metal on a large scale, in which 240 kilogrammes of mother-liquor, obtained from 44,200 kilogrammes of water, was employed. The difficulty of working up the immense mass of raw material used in the preparation of the cæsium-salts described in the present memoir, was lessened by the kindness of Dr. Gundelach, who arranged that the rougher portions of the operations should be conducted in an alkali-works, in the following manner.

The mother-liquors were treated with sulphuric acid in a salt-cake furnace, and the sulphates thus obtained boiled for some time with water containing caustic lime. The excess of lime was separated from the aqueous solution by means of oxalic acid, and then the greater part of the sulphuric acid precipitated with nitrate of barium, whilst the last portions, together with the soluble magnesian salts, were removed by the addition of hydrate of barium. The filtered liquid, neutralized by nitric acid, yielded on evaporation a salt which was worked up in the Heidelberg laboratory.

On treatment with strong alcohol, this mass yielded a residue weighing 6·5 kilogrammes, which was tolerably rich in cæsium salts, and was subjected to a series of processes which will be described under the head of "Residue No. 1."

To the alcoholic extract from the original mass a concentrated aqueous solution of carbonate of ammonium was added in order to precipitate the greater portion of the lithium-salts. The solution was then evaporated to dryness in an iron vessel and heated to expel all ammoniacal salts, the brown mass (containing much oxide of iron) dissolved in water, and the aqueous solution evaporated to dryness. On extraction with alcohol, this salt yielded a residue which we will call "Residue No. 2," to the treatment of which we shall recur.

This second alcoholic extract gave with bichloride of platinum a yellow precipitate, which weighed 8·5134 grms. after washing with water. The precipitate did not undergo any change of composition on boiling with water; and when it was placed in the spectrum-apparatus, showed the cæsium and rubidium lines with great intensity. It therefore consisted almost entirely of a mixture of the double chlorides of cæsium and rubidium, and platinum.

These 8·5134 grms. = A lost by reduction in a current of hydrogen 1·8719 grm. = B. The residue contained metallic platinum, and the neutral chlorides of rubidium and cæsium. If we call x the quantity of the double chloride of rubidium and platinum, and y the quantity of the chloride of cæsium* and platinum, we have then

$$x+y=\mathrm{A},$$

$$\frac{2\mathrm{Cl}}{\mathrm{Pt}+\mathrm{Rb}+3\mathrm{Cl}}x+\frac{2\mathrm{Cl}}{\mathrm{Pt}+\mathrm{Cs}+3\mathrm{Cl}}y=\mathrm{B};$$

hence we find

$$x=35{\cdot}4975\,\mathrm{B}-7{\cdot}65588\,\mathrm{A},$$
$$y=8{\cdot}6559\,\mathrm{A}-35{\cdot}4975\,\mathrm{B}.$$

By substitution of the values of A and B, we have the following as the composition of the precipitate:—

Double chloride of platinum and cæsium . .	1·2701
Double chloride of platinum and rubidium . .	7·2433
	8·5134

Hence 100 parts of the alkaline chlorides, combined with the chloride of platinum, are composed of—

Chloride of cæsium . . .	16·93
Chloride of rubidium . .	83·07
	100·00

* The atomic weight of the cæsium is taken to be 123·35, according to determinations which will be found in the sequel.

Residue No. 2 from the second extraction with alcohol, when dissolved in water and treated with bichloride of platinum, gave a yellow precipitate, which, after being boiled out from ten to twelve times, weighed 23 grms. 13·83 grms. = A of this precipitate lost, on reduction in hydrogen, 3·182 grms. = B. Hence the whole 23 grms. consisted of,—

Double chloride of platinum and cæsium . .	11·76 grms.
Double chloride of platinum and rubidium . .	11·24 „
	23·00 „

100 grms. of the alkaline chlorides contained in the precipitate were, therefore, made up of—

Chloride of cæsium . . .	54·89 grms.
Chloride of rubidium . . .	45·11 „
	100·00 „

The residue No. 1 weighed 6·5 kilogrammes, and for the most part consisted of the chlorides of potassium and sodium. In order to obtain the cæsium-salts still contained in it, the mass was dissolved in water, and the boiling solution precipitated with a quantity of bichloride of platinum amounting to only from 8 to 10 thousandths of the weight of the total mass. By boiling out the platinum precipitate fifteen to twenty times with water, and by adding the boilings to the original solution until they become slightly yellow-coloured, a second platinum precipitate is obtained, which must be treated in the same way as the first. The platinum precipitates are thus boiled out until no further deposit of a light-yellow insoluble crystalline mass is observed, and then all the precipitates, thus purified by washing, are reduced in a current of hydrogen, and the soluble salts extracted with water. The aqueous solution contains a mixture of chloride of cæsium and chloride of rubidium.

A kilogramme of the residue thus treated yielded 1·0348 grm. of such a mixture of the chlorides of rubidium and cæsium, from which nitrate of silver precipitated 1·1404 grm. of chloride of silver. Let A_1 signify the mixture of x_1 parts of chloride of rubidium and y_1 parts of chloride of cæsium; and let B_1 signify the weight of chloride of silver obtained from the salt A_1; we then find the values of x_1 and y_1 from the following equations:—

$$x_1 = 3{\cdot}50963\,B_1 - 3{\cdot}16906\,A_1,$$
$$y_1 = 4{\cdot}16906\,A_1 - 3{\cdot}50963\,B_1.$$

By help of these equations, and of the values of A_1 and B_1, it is easy to see that the residue No. 1, weighing 6·5 kilogrammes, contains—

Chloride of cæsium . .	2·0267 grms.
Chloride of rubidium . .	4·6995 „
	6·7262 „

Or 100 parts of the mixed chlorides contain—

Chloride of cæsium . . .	30·13
Chloride of rubidium . .	69·87
	100·00

Taking a mean of all these experiments, we find that the mother-liquor from 44,200 kilogrammes of the Dürkheim water yielded altogether—

9·237 grms. of chloride of rubidium,
7·272 grms. of chloride of cæsium.

These determinations do not, of course, profess to be very accurate. The numbers thus obtained are, however, correct enough for us to be able to give an approximate value for the quantity of the rubidium and cæsium compounds contained in the Dürkheim mineral water. The following numbers express the composition of 1000 parts of this remarkable mineral water, according to analyses made in the Heidelberg laboratory:—

Mineral Water of Dürkheim.

Bicarbonate of calcium	0·28350
Bicarbonate of magnesium	0·01460
Ferrous-bicarbonate	0·00840
Manganous-bicarbonate	trace
Chloride of calcium	3·03100
Chloride of magnesium	0·39870
Chloride of strontium	0·00810
Sulphate of strontium	0·01950
Chloride of sodium	12·71000
Chloride of potassium	0·09660
Bromide of potassium	0·02220
Chloride of lithium	0·03910
Chloride of rubidium	0·00021
Chloride of cæsium	0·00017
Alumina	0·00020
Silica	0·00040
Free carbonic acid	1·64300
Nitrogen	0·00460
Traces of sulphuretted hydrogen . .	0·00000
Traces of phosphates	0·00000
Traces of ammoniacal salts	0·00000
Traces of indeterminate organic bodies.	0·00000
	18·28028

The mother-liquors obtained from the Dürkheim salt-works, and sold for the purpose of manufacturing brine-baths, were found to contain the new alkaline chlorides in a more concentrated form, as is seen from the following analysis:—

Mother-liquor from Dürkheim Waters, composition in 1000 *parts.*

Chloride of calcium	296·90
Chloride of magnesium	41·34
Chloride of strontium	8·00
Sulphate of strontium	0·20
Chloride of sodium	20·98
Chloride of potassium	16·13
Bromide of potassium	2·17
Chloride of lithium	11·09
Chloride of cæsium	0·03
Chloride of rubidium	0·04
	396·88

The mother-liquors of the brine-springs of Kreuznach, Kissingen, and Nauheim were likewise found to contain evident traces of rubidium and cæsium compounds, as is seen by the following analyses made in the Heidelberg laboratory:—

Mother-liquor of Brine from Kissingen, composition in 1000 *parts.*

Chloride of magnesium	189·59
Sulphate of magnesium	36·01
Chloride of sodium	41·37
Chloride of potassium	18·72
Bromide of potassium	10·62
Chloride of lithium	12·85
Chloride of cæsium	trace
Chloride of rubidium	trace
	309·16

Mother-liquor of Brine from Theodorshall near Kreuznach, composition in 1000 *parts.*

Chloride of calcium	332·39
Chloride of magnesium	32·45
Chloride of strontium	2·86
Chloride of sodium	3·44
Chloride of potassium	17·12
Bromide of potassium	6·89
Iodide of potassium	0·08
Chloride of lithium	14·53
Chloride of cæsium	considerable trace
Chloride of rubidium	trace
	409·76

The salt which crystallizes from this mother-liquor appears to be free from cæsium and rubidium; it is, however, remarkable for the large quantity of chloride of strontium which it contains. According to an analysis made in the Heidelberg laboratory by

M. Sieber, the salt possesses the following per-centage composition :—

Chloride of calcium	54·28
Chloride of magnesium . . .	2·76
Chloride of strontium . . .	11·19
Chloride of sodium	2·01
Chloride of potassium . . .	7·98
Water	21·78
	100·00

From the foregoing analyses, it would appear that cæsium and rubidium occur pretty generally in the water of brine-springs. These metals, however, are likewise found to be present in the waters of the non-alkaline springs containing but small quantities of soluble salts. Thus, we have proved the presence of the salts of these two new alkalies in two of the hot springs at Baden-Baden, namely in the Ungemach and Höllenquelle.

The former of these springs was found to possess the following composition in 1000 parts :—

Water of the Ungemach hot spring at Baden-Baden.

Bicarbonate of calcium . .	1·475
Bicarbonate of magnesium .	0·712
Ferrous bicarbonate . . .	0·010
Manganous bicarbonate . .	trace
Sulphate of calcium . . .	2·202
Sulphate of strontium . . .	0·023
Sulphate of barium . . .	slight trace
Chloride of calcium . . .	0·463
Chloride of magnesium . .	0·126
Chloride of sodium . . .	20·834
Chloride of potassium . . .	1·518
Bromide of potassium . . .	trace
Chloride of lithium . . .	0·451
Chloride of rubidium . . .	0·0013
Chloride of cæsium . . .	trace
Silica	1·230
Alumina	0·001
Combined nitric acid . . .	0·030
Combined ammonia . . .	0·008
Combined arsenic acid . .	trace
Combined phosphoric acid .	trace
Combined oxide of copper .	trace
Indeterminate organic bodies	trace
Free carbonic acid	0·456
	29·552

The salts of cæsium and rubidium, as well as those of lithium and strontium, have been likewise found in the water of the Wiesbaden springs, as also in that of the newly-bored artesian well at Soden near Frankfurt. In order to obtain evidence of the presence of the new alkalies in this water, it is only necessary to boil out the platinum precipitate obtained from the mother-liquor of 6 to 8 litres of the water; the cæsium and rubidium lines are then easily recognized in the spectrum-apparatus.

We have examined small quantities of the ashes of land-and sea-plants, as well as Chili saltpetre and other alkaline salts occurring in commerce, for the compounds of the new alkaline metals, but we have not succeeded in detecting, in these substances, the presence of the salts of either metal.

Having thus considered the occurrence and diffusion of cæsium, we pass on to the consideration of the methods of separation, by means of which the compounds of this metal can be obtained in a state of purity. If, as is almost always the case, potassium, rubidium, and cæsium occur together with sodium and lithium, the first three metals can be separated from the two latter by means of bichloride of platinum. The double chloride of platinum and potassium can be separated from the platinum compound of the two new alkaline chlorides, as has been described, by boiling the double salts out about twenty times with small quantities of water; and thus the more soluble potassium-salt may be almost entirely removed. The double platinum compounds, which now contain but traces of potassium-salts, are next heated to redness in a current of hydrogen, at which temperature the chlorides of cæsium and rubidium do not fuse. The mass is then treated with about seventy times its weight of water, and the alkaline chlorides thus dissolved. The residual platinum is again converted into chloride, which is diluted with water to the same bulk as the solution of the alkaline chlorides; both solutions are then heated to boiling and mixed together. As soon as the precipitate which forms on mixing the solutions has collected in sufficient quantity by the cooling of the liquid, it is thrown on a filter, dried, and again subjected to the same treatment of reduction in hydrogen, &c., until a small portion brought into the spectrum-apparatus shows at most a faint trace of the potassium line Kα. The precipitate then contains solely the chlorides of cæsium and rubidium. For the purpose of separating these two bodies, the solubility of the carbonate of cæsium in absolute alcohol, and the insolubility of carbonate of rubidium in the same liquid, is made use of.

The separation of the carbonate of cæsium by repeated extractions with alcohol is, however, a difficult operation, as a double

carbonate of rubidium and cæsium appears to be formed which is not perfectly insoluble in alcohol. On this account, we prefer to prepare the caustic alkalies by the action of baryta-water on the sulphates, and then to convert about one-fifth part of the mixed caustic alkalies into carbonate by evaporation with carbonate of ammonium in a silver basin. The hydrated oxide of cæsium is then dissolved out from such a mixture by alcohol, leaving a residue consisting of carbonate of rubidium containing cæsium. If this mode of separation be repeated five or six times, each time taking care to use as little alcohol as possible, the hydrated oxide of cæsium is obtained quite free from rubidium, as may be proved by examination in the spectrum-apparatus. It is scarcely necessary to remark that the numerous residues which accumulate in the course of the several operations must again be worked up in the same way as the original substance, and that the platinum can be used over and over again without any great loss.

The following experiment served as a preliminary determination of the atomic weight of cæsium. Chloride of cæsium was prepared from the mixed chlorides freed from all potassium salt, by the methods just described, and the contained chlorine estimated as chloride of silver.

0·5219 grm. of the chloride of cæsium yielded 0·4995 grm. of chloride of silver.

The chloride of cæsium of the first preparation was submitted a second time to the purifying treatment above described. 1·7690 grm. of the third preparation yielded 1·6548 grm. of chloride of silver. This substance was again purified in like manner, and 0·3727 grm. of the third preparation yielded 0·3402 grm. chloride of silver. After the process of purification had been repeated for a fourth time, 1·3860 grm. of the substance yielded 1·2518 grm. of chloride of silver. After the fifth time of treatment, 1·0124 of the salt gave 0·9144 grm. chloride of silver; and lastly, after the sixth purification, 0·4126 grm. of chloride of silver was obtained from 0·4572 grm. of the salt.

Hence 100 parts of the substance under examination gave—

After the first	purification	95·708 grms.	of chloride of silver.
„ second	„	93·486	„
„ third	„	91·280	„
„ fourth	„	90·318	„
„ fifth	„	90·320	„
„ sixth	„	90·245	„

It is evident from these numbers that, after extracting the salt four times with alcohol, a substance was obtained which on a repetition of the process did not undergo any alteration in composition.

If we calculate, then, from the three last experiments the atomic weight of cæsium, we have—

	123·31
	123·31
	123·44
Mean . .	123·35

As many of the salts of rubidium and cæsium are isomorphous with potassium-salts, the number 123·35 cannot be regarded as a multiple or submultiple of the atomic weight of cæsium. Hence we draw the remarkable conclusion, that the new metal possesses the largest atomic weight of all the known elementary bodies with the exceptions of gold and iodine.

[To be continued.]

XLIII. *Considerations on the Phenomena attending the Fall of Meteorites on the Earth.*—Part I. *By* W. Haidinger, *For. Mem. R.S. L. & E., and Director-General of the School of Mines in Austria* *.

THE only falls of meteorites which I propose to take into consideration in this paper, are those which have been observed as accurately as possible. Generally in such cases dates and particulars that can be perfectly relied upon are not common. The phenomena taking place without warning and occupying so short a period of time, it is only from persons accustomed to receive impressions promptly that we can obtain trustworthy observations. Dr. Gustavus Scheffczik, whilst on a hunting-excursion, saw near Strakowitz in Bohemia, on the 29th of November, 1859, at $10^h\ 45'$ A.M., a luminous star-like point appearing suddenly on a clear sky, due north 15° zenith-distance, increasing gradually to an intensely luminous globe, equal in size to one-third of the full moon (about one-third of her diameter), and passing along a parabolic path towards S. 60° E. When under an angle of altitude of 25° (azimuth =65°) it assumed an oval shape, the pointed end bent downwards and forwards, and lastly, apparently dissolved into many large sparks, one of which evidently fell down in a vertical direction. Dr. Scheffczik estimates the phenomenon to have lasted from four to five seconds. A noise as if myriads of birds were flying through the air attracted his attention. According to his watch, $1\frac{1}{4}$ minute passed in silence after the disappearance of the luminous appearances; then followed, quickly after each other, four detonations (the last the most intense), resembling the

* A translation by Count Marshall, of a paper read before the Imperial Academy of Sciences at Vienna, on the 14th of March, 1861. Communicated and revised by R. P. Greg, F.G.S.

discharges from heavy ordnance. The ground shook under the observer's feet, as was corroborated by three other gentlemen in company with Dr. S., who likewise heard the four detonations.

I have received some other observations concerning the same phenomenon; but I consider Dr. Scheffczik's observation most valuable, from the excellent account it gives of the first approach of the "star-like" luminous body, and of its subsequent progression.

Scarcely any fall of aërolites has ever been so exactly and fully observed as that which fell at New Concord, Muskingum Co., Ohio, on May 1st, 1860*. Professor Evans, of Marietta College, Ohio, calculated several elements of the orbit. The meteor, first seen as a fiery globe at a horizontal distance of 20 to 30 (English) miles, appeared like the full moon. An altitude of 40 miles, derived from other observations, would give to it a real diameter of $\frac{3}{8}$ths of a mile. It moved from S.E. to N.W. The final velocity was about 4 miles a second. Nearly thirty stones, of about 700 lbs. total weight, were found to have fallen; the largest of them, weighing 103 lbs., is now in the Museum of Marietta College. All these stones taken together would fall far short of the apparent size of the meteor, as is the case with many other observations of a similar nature, especially with that of Agram in Croatia, May 26th, 1751, where two masses of native iron, the one of 71 lbs., the other of 16 lbs., were the only material residuum of a meteor whose apparent diameter was scarcely under 3000 feet†! At first the New Concord stones were warm, so that particles of the moist ground on which one of them had fallen, soon dried up, at least in the case of one weighing 71 lbs.

Its greatest heat was not more than that which the stone would have had if exposed for some time to the natural heat of the sun's rays. The largest of the stones (103 lbs.) was found about three weeks after the fall, beneath the root of an oak tree. It had gone through another root in an oblique direction, and had penetrated to the depth of nearly 3 feet into a hard argillaceous ground: no mention is made of its probable temperature at the time of falling. Those who witnessed the fall, only perceived that the stones were "black," they did not mention the appearance of any fireball‡. At the moment of the fall they

* See Silliman's American Journal, vol. xxx. for July 1860.

† See "Der Meteorstein-Fall v. Hrashina bei Agram, 26 Mai, 1751," by W. Haidinger, Proceedings of the Vienna Imperial Academy, Class of Mathematics and Natural Sciences, 1859, vol. xxxv. p. 361 (283).

‡ "No one of the many persons who saw the stones fall, and who were in the immediate vicinity at the time, noticed anything of the luminous appearance described by those who saw it from a distance."—*Silliman's Journal.*

heard hissing sounds, and that before the chief detonation had attracted their attention. All the stones were covered with a black crust bearing evidence of fusion, and presented angular and fragmentary shapes; their interior resembled grey solid rocks*. The American naturalists inferred from the collated accounts about the igneous globe, the acoustic phenomena, and the fall itself, that the first and chief detonation took place at an altitude of about 40 miles (English) above the southern portion of Noble County, at a distance of about 30 miles from New Concord; and that the fall of the stones themselves commenced about one mile S.E. of that place, extending over an area of 10 miles in length by 2 to 3 in breadth, the largest ones falling last. The sound perceived was supposed to have been explosive in its nature; and the meteor, after having ceased to be visible, must have continued its course towards the North-west. These are some of the most important facts relating to the phenomenon. Desirable as it is to pursue induction step by step, it is impossible to give a clear exposition without sketching previously the succession in time of each event as they are observed and perceived by our senses. Nobody who has ever examined meteorites with more than superficial attention, can have doubted that their interior and their exterior present two different periods of formation. The general form of meteorites is that of *fragments*, the constitution of their external crust is the consequence of *superficial fusion*. They are fragments coming from remote cosmical regions, which having entered the earth's atmosphere, are first perceived by us as stars, increasing in size as they come nearer to us. Great care should be taken to observe and note the moment or time with as much exactitude as possible, as, combined with the time of the year and the hour of the day, it gives us the direction of the meteor. The direction and the velocity of our globe in its circum-solar orbit (19 English miles per second, while a point on the equator by diurnal rotation moves 1464·7 Vienna feet in a second, or 900 nautical miles per hour), are well known.

Many observations have proved meteorites to travel 16 to 40 English miles in a second. Humboldt, in his 'Cosmos,' has even, from the observations of J. Schmidt, Heis, and Houzeau, calculated a velocity of 95 miles a second.

These orbits cross and oppose each other in every conceivable direction. Important consequences may be deduced from these enormous velocities, as compared with what takes place on the surface of our globe. A devastating hurricane takes place in

* "Viewed from most positions, this stone (that of 103 lbs. at Marietta College) is angular, and appears to have been quite recently broken from a larger mass."

our atmosphere whenever an air-current is progressing at the rate of 92 miles (English) per hour. A point on the equator, by diurnal rotation progresses at the rate of 1464·7 Vienna feet per second without disturbance of the atmospheric equilibrium, on account of the general atmospheric pressure being nearly equal in places lying very near each other. According to Sir John Herschel, the movement of a "devastating hurricane" is equivalent to a pressure of 37·9 lbs. Vienna (32·81 lbs. English) weight, on one square foot Vienna measure. The atmospheric pressure (=32 feet of water) on one Vienna square foot is 1804·8 lbs., or, compared with that of the most powerful hurricane, as 55 to 1.

I am glad to see these details, as I give them from sources most within reach, confirmed by Prof. E. E. Schmidt's, of the University of Jena, in his copious and excellent 'Manual of Meteorology' (vol. xxi. of the *Allgemeine Encyklopädie der Physik*), edited by Dr. G. Kersten and other eminent physicists). The following synoptic Table, calculated by Mr. Rouse (Report of the Tenth Meeting of the British Association, held at Southampton in Sept. 1846, p. 344) is found on page 483 of Prof. Schmidt's Manual:—

Velocity of wind.		Pressure per square foot in lbs. avoirdupois.	Character of wind.
English miles per hour.	English feet per second.		
60	88·02	17·715	Great storm.
80	177·36	31·490	Hurricane.
100	146·70	49·200	Destructive hurricane.
913–916	1340·0	1 atmosphere.	

The wind-scale of the Smithsonian Institute (published by the Smithsonian Institute, Nov. 1853, Washington, p. 173) offers analogous results.

It is the best proof in favour of the use of such extensive and elaborate synoptic works as Prof. Schmidt's Manual, that they gave me complete confidence in the *data* I had so laboriously compiled from other sources, and this, thanks to the author's kind attention to me, just at the moment when I felt most in want of such.

What is the state of the single particles of air composing our atmosphere in the elevated regions, where meteorites, first entering it, are capable of producing luminous phenomena as intense as observed at New Concord, even at the enormous altitude of 40 English miles? In these elevated regions the temperature may probably not exceed that of the interplanetary space, *i. e.* 100° Reaumur. Movements of particles may be supposed indeed to take place in the higher regions

of the atmosphere, as on these depend the changes of atmospheric pressure nearer to the earth's surface, the causes of the winds, &c. Whenever solid bodies move through them, so abnormal an event goes on with such enormous rapidity, that these particles, quite isolated from each other, must be positively pushed aside. In the van of the progressing meteorite a stratum of atmospheric particles is formed, having no time to escape before the progressing body, but by streaming back alongside of it. The velocity of a meteorite, supposed on an average to be seven German miles (24,000 feet) per second, is to that of a hurricane of 134·72 feet per second as 124·4 to 1. Suppose the pressure to increase in the same proportion, it would be per square foot, for the hurricane =32·8 lbs., and for the meteorite 4080·32 lbs., or more than 22 atmospheres.

It may be supposed that such a sudden compression (action and reaction continually remaining equal) must have the same effect as the compression of air in the old tinder-boxes alluded to by Prof. Benzenberg. It might not here be out of place to quote *in extenso* a passage from a book published fifty years ago*, expounding views still far from being cleared up:—

"The incandescence perceived around fireballs in a state of ignition may be the result either of *combustion*, although with difficulty admissible in air so very rarefied, or of *friction*, as generally believed. I think it results still more from the *compression* of air, as in our newly invented tinder-boxes air produces fire by mere compression. Could not *electricity* become free in the same way? Suppose a cubic mile of air to be suddenly compressed to a volume of one cubic foot, would not then the electricity originally contained in it be set at liberty? The circumstances attending the explosion of igneous globes seem to be in accordance with this supposition. These globes, when first seen, do not appear larger than bright stars; as they approach the terrestrial surface (generally in an oblique direction) they increase to the size of the full moon, and at last, when at a few miles distant, explode with a violent detonation. The cause of this explosion is probably an excessive accumulation of electric matter, streaming from compressed air into the igneous globe of about 3000 feet in size of (?) metallic substance. The distance being still too considerable to admit of a discharge to the earth, this takes place in the open air, or within a cloud.

"Probably the place of the discharge depends less on the proximity of the terrestrial surface than on the density of air regulating the *maximum* of compression and accumulation of electricity. Subsequently to the explosion, the single fragments

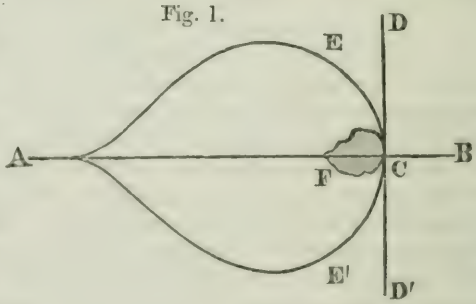
* *Briefe geschrieben auf einer Reise durch die Schweiz im Jahre* 1810, von J. F. Benzenberg. 1 vol. Düsseldorf, 1811.

fall to the surface of the earth, with a velocity probably inferior to that of a bullet shot from a gun, the air increasing in density as it becomes nearer the surface of the globe, resistance increasing proportionally, so that there may be but a slight difference in the final velocity, whether the body fall from a height of one or of five miles."

I have here quoted views somewhat opposed to those which I myself intend to propose (as those relating to the explanation of the explosion of fireballs); yet some of the above-quoted assertions may perhaps be worth further consideration.

The following exposition of the way in which this may occur may not be altogether devoid of probability. Compression, first of all, developes heat and light. Immediately in front of the meteorite is formed a centre of expansion, from which the compressed air tends to expand in every direction. Whatever lies in the direction of the orbit, is left in the rear of the progressing meteorite; whatever lies opposite to it, contributes to the fusion of the superficial crust, or by its resistance either retards its progress, or gives rise to a rotatory movement around an axis coinciding with the meteorite's orbit, even if it should have undergone such a motion only on entering the terrestrial orbit. A part of the air made luminous by compression, is forced out as at C, in every direction perpendicularly to the orbit AB (fig. 1). Resistance continues against this luminous disc, forces it backwards, overcomes it gradually at some distance from the centre towards EE', and rounds it off behind the meteorite in the shape of an igneous globe, either round, or as frequently happens oviform; occasionally extended so far back as to form even an actual tail.

Fig. 1.



Instances of two or more luminous bodies behind each other have been observed, as those seen at Elmira, Long Island, United States, July 20, 1860; at Littau in Moravia, end of August 1848 or 1849; at Collioure in France, February 21st, 1846. In these cases we may suppose that the single fireball first seen contained already a certain number of fragments, acted upon differently by the resistance of the air, according to their differences in size, shape, and perhaps specific gravity, so that the

heavier among them found less obstruction in pursuing their way than the lighter ones.

M. Julius Schmidt observed at Athens, July 27th, 1859, a magnificent green meteor, moving slowly in twelve seconds through an arc of 28°, commencing with a faint light, and ending as faintly, while about the middle of its course it expanded into a ball of 8–10 minutes in diameter, casting an intense light over the whole town and neighbouring hills.

An orbit having its convexity turned towards the earth's surface, as that of the meteor of 20th July, 1860, seen in the United States, may be indicative of a degree of specific gravity inferior to that generally the case in meteorites. In this case the motion of meteorites may become slower and slower, and at last be completely stopped; while there is little chance of their again returning into the cosmical space, from whence they entered our atmosphere.

The meteorite in question evidently entered the more rarefied strata of the atmosphere, and, perhaps influenced by the short duration of the igneous globe surrounding it, continued on its course into space. Its speed, though somewhat diminished, was certainly not annihilated.

Hitherto we have left out of consideration the altitude of the atmospheric strata in which a meteor is supposed to move; nor is this omission objectionable: suppose the meteorite moved along close to the surface of the earth under the pressure of a whole atmosphere, answering to a column of mercury 30 inches in height, and at the rate of seven miles per second, it would act on every square foot of resisting air with a pressure of 22 atmospheres*; this pressure would only amount to 11 atmospheres at a height of between 18,000 and 19,000 feet, where the barometer would indicate an atmospheric pressure of only 15 inches. It must, however, not be lost sight of, that under such circumstances the resistance of the surrounding air is also notably diminished, and that consequently the atmospheric particles forcibly driven out before the centre of elasticity will find the same facility for streaming along, or flowing in the directions C D, C D′ (fig. 1).

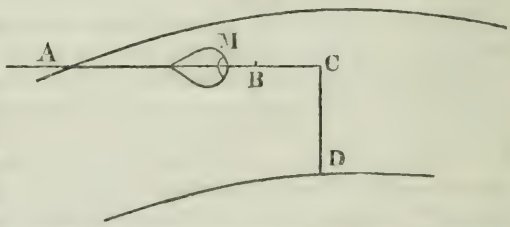
If electric tension in the extremely rarefied strata of our atmosphere is really as energetic as it is generally admitted to be, we are entitled to suppose a high development of electrical light. The expressions used by Benzenberg in the above-quoted passage, suggest no idea adequate to our present mode of viewing this subject. The view recently enounced in a totally different direction by one of the first of living physicists, Professor Plücker, seems to be in exact accordance with the subject considered here.

* See E. E. Schmidt's *Lehrbuch der Meteorologie*, page 913.

In his paper on the constitution of the electric spectra of certain gases and vapours (see Poggendorff's *Annalen*, 1859, vol. cvii. p. 505), the illustrious Professor says, "What is the thing emitting light when an electrical discharge takes place through the narrow passage of a Geisslerian tube, as much exhausted of air as possible, and including gas or vapour? There is no light unless some ponderable substratum emits it; *there is consequently no electrical light in the abstract sense of the word.* All my observations have confirmed me in this persuasion. But how is electricity acting here on the gaseous particles? In my opinion only as an exciter of heat. *The gaseous particles become incandescent.* The thick glass in the narrower portion of the Geisslerian tube is very notably heated when the discharge from Ruhmkorff's apparatus passes through the gas contained in it. If, then, the heat transmitted to the glass from the dispersed gaseous particles, whose tension is often measurable by fractions of millimetres, increases to a notable degree, to what a degree of intensity must these particles be heated!"

The cosmical orbit of the meteorite M entering the terrestrial atmosphere A (fig. 2) terminates at C; from this moment the meteorite belongs to our earth, and falls straight down from C (where, after exploding, its light is extinguished) to D on the earth's surface. The line C D represents its terrestrial orbit.

Fig. 2.



Terrestrial attraction is quite an insignificant element compared to the planetary or cosmical impulsion peculiar to any meteorite; and but for the resistance offered by the atmosphere, few or no meteorites would reach the surface of our earth, except those whose orbits were directly aimed towards it.

Meteoric stones after falling appear black; and their enamelled crust proves them to have undergone superficial fusion from exposure to high temperature. Their interior is frequently not more heated than would permit of their being held in the hand without inconvenience.

This is no matter for surprise, as the stone having quite recently come from the cold regions of interplanetary space, a compensation may be supposed to take place between the outside and the inside.

Fragments of the Dhurmsala meteorite (Punjab, 14th July,

1861), probably from the extreme cold of the interior, showed, at the moment of their fall, a temperature notably below congelation*. Meteoric *iron*, however, being a good conductor of heat, comes down far more heated, and even in a state of intense incandescence, as was the case with the iron of Corrientes in Caritas Paso, near the river Mocorita, in January 1844, mentioned by Mr. Greg†, which fell at 2 A.M. in the shape of a lengthened globe, a fiery streak marking its passage through the air. The mass fell down at a distance of about 1200 feet from Mr. Symonds, who indeed first made known this fall. Later in the morning it could not be approached nearer than ten or twelve yards, on account of the heat emanating from the mass, which projected several feet out of the ground. During the fall the atmosphere was evidently in a state of motion, as if repelled by the falling body, producing a whirlwind of short duration.

This description is quite consistent with the facts to be derived from the preceding considerations. In this case the meteoric iron-mass struck the earth nearly point blank, falling under an acute angle of 60°.

Another very characteristic phenomenon connected with the vanishing of meteoric light, is the accompaniment of intense explosive *sound*, resembling the ignition of gunpowder fired from guns or mines. Generally one detonation is strikingly loud, frequently followed by others of a "rattling" character. The meteor "explodes," as it is commonly called, and lets fall from it one or more stones, disproportionally small in quantity as compared with the probable size of the fireball itself.

What could then have become of a body so luminous as that of a large meteor, which, according to Prof. Laurence Smith's experiments, might indeed appear far larger than the solid matter contained in it could justify one in supposing possible?

According to Prof. Smith's experiments—made, 1st, with the electric light between carbon points; 2nd, with the oxy-hydrogen light falling on lime; and 3rd, with the light from steel burning in oxygen,—the irradiation of a luminous point gives the following numbers for the apparent size at four different distances:—

Distance...	10 inches.	600 feet.	1320 feet or ½ mile.	2640 feet or ¼ mile.
1. Carbon	0·3 line	½ } Diameter	3 } Diameter	3½ } Diameter
2. Lime ...	0·4 „	⅓ } of the	2 } of the	2 } of the
3. Steel....	0·2 „	¼ } moon.	1 } moon.	1 } moon.

Though persons struck by any uncommon sight are generally

* Proceedings of the Imperial Academy of Vienna, sitting of the 29th Nov. 1860.

† Philosophical Magazine for July 1855.

inclined to overrate the size of an object, the reports or accounts of fireballs showing a half or the whole of the full-moon's diameter, when seen from a distance of 20, 40, 60, or 100 miles, cannot, however, entirely rest on self-delusion*. A large space may be occupied by the igneous globe, surrounding a far smaller nucleus, consisting of one or more fragments.

On coming with enormous velocity from planetary space into our atmosphere, the acoustic phenomenon may be accounted for by supposing that the fireball includes, as we have attempted to explain, a real vacuum maintained by the *resistance* of the *atmosphere* against it. The original velocity having at length been sufficiently retarded by the air, the meteor becomes almost stationary; at this moment the vacuum suddenly collapses in the already rather dense air, and detonation ensues from repercussion of the air filling up the vacuum. The intensity of the sound ceases to be a matter of wonder when we consider the explosions caused by setting fire to bubbles filled with oxy-hydrogen gas suspended in the air. The so-called "consecutive explosions," or series of smaller detonations, may depend on the more or less gradual diminution of the cosmical velocity†.

Hitherto only *one* solid body has come into question. When, however, meteorites arrive in flocks or groups, as when 3000 stones (the largest 17 lbs.) fell from a detonating meteor at L'Aigle in France, on the 26th of April, 1803, nearly 200 at Stannern in Moravia, 22nd May, 1808, and some 30 or 40 near New Concord, Ohio, on May 1st, 1860, it may be supposed that even if one principal explosion "had commenced the action," subsequent detonations of the several isolated portions could likewise have taken place. I do not, however, believe that in the above-

* The meteor of Feb. 11, 1850, seen in England at a distance subsequently calculated at 50 miles, appeared, as at Hartwell, as large as the full moon; at places 100 miles distant from its vertical passage, as large as Venus. That of July 20, 1860, seen in the United States, had a decided apparent diameter nearly equal to that of the full moon, when at a height of 41 miles. That of October 18, 1783, at a height of 60 miles, over Lincolnshire, presented a similar appearance.—R. P. G.

† It may be here fair to mention that Mr. Benj. W. Marsh, of the United States, considers (in his Report in the Journal of the Franklin Institute, on the daylight meteor of Nov. 15, 1859, seen in New Jersey) that the aërolitic *detonation* arises from a series of decrepitations caused by the sudden expansion of the surface of the stony fragment, the whole time of flight not being sufficient to penetrate the mass. At the forward end these explosions would take place under great pressure, which might account for the loudness of the sound. The force of these explosions, directed backwards, would likewise tend to check the forward velocity of the mass. He also considers that the audible explosion, often lasting several minutes, is the result of the actual bursting of the meteor; for though the explosion might only occupy in reality half a second of time, yet in that interval the noise might be distributed over a distance of twenty or thirty miles.—R. P. G.

cited instances the stones were formed by the bursting or explosion of one large stone, but that they actually entered the atmosphere as a group or swarm of separate individuals, surrounded, as I have ventured to suggest, by what appears to us the luminous fireball*. I must here shortly allude to some peculiarities common both to *stone* and iron meteorites. One is the "pitted" or indented appearance usually presented on their surfaces. This "pitted" surface is particularly evident on the meteoric stone of Gross-Divina, which fell July 24th, 1837, in Hungary; and in the meteoric iron of Nebraska (Transactions of the Acad. of Sciences of St. Louis, vol. i. no. 4, plate 21). They are best developed on the side supposed to have lain backwards (see F in fig. 1). The side turned towards C is constantly more uneven and rough, as though it had pressed against a homogeneous mass of air, while air-currents may, like pointed flames, turn alternately towards the plane F. Marginal seams, as on the stones of Stannern, owing to the fusibility of the crust, give place to similar conjectures. As for the general form, the centre of gravity must have been in the forepart or front, as long as the meteorite was moving through space. When rotation round an axis had once commenced, and become accelerated in consequence of the propulsory movement diminishing, the point next in gravity must have taken its place in the plane of rotation, so that an iron mass of a flat form, as that of Agram is, could be propelled lying on its flat side. This iron is indeed of very different aspect on each of its broader planes; the rougher of them was certainly directed forward, as long as propulsion continued, the smoother surface remained turned backward, and not acted upon by external agents. The flat shape of the whole characterizes the Agram iron as having originally filled up a vein-like narrow cavity.

A disruptive explosion is only indubitable where, as in the stone-fall of Pegu (December 27, 1857), two fragments of the same stone, fitting each other exactly, have been found at a certain distance (in the case in question, 10 English miles !)†; such a disruption may cause a sound, as would a millstone under analogous circumstances, but certainly of less intensity

* See Haidinger on "eine Leitform [typical form] der Meteoriten," Vienna Acad. Proceedings, vol. xl. 1860, page 525, note. It yet by no means seems proved that meteorites do enter our atmosphere in groups, and that then an explosion again scatters them as they fall to the earth; it seems more probable, and certainly as possible, on the other hand, that one large friable mass, constituting probably the nucleus of the *single* fireball, as that of L'Aigle, bursts into many pieces, sometimes, no doubt, into hundreds of small fragments, as well as occasionally into the finest dust.—R. P. G.

† Haidinger, vol. xlii. p. 301 of the Proceedings of the Imperial Academy of Vienna, "Die Meteoritenfälle von Quenggouk bei Bassein in Pegu."

than that caused by the sudden collapse of the vacuum within a large fireball.

I have left unnoticed many other particulars concerning this class of phenomena, as well as attempts at explanations, and the views of others respecting them; and I even abstain from mentioning their connexion with M. Coulvier-Gravier's long-continued and accurate investigations. Meantime I have received through the editor's particular kindness, a copy of Dr. Laurence Smith's paper on the late fall of stones at New Concord, before referred to in this paper, and published in Prof. Silliman's American Journal (Jan. 1861, vol. xxxi. p. 87). In a letter addressed to me, Dr. Smith, for a long time a most careful investigator of meteorites, writes as follows:—"The method hitherto used in studying meteorites is still very deficient. To obtain tolerably accurate notions concerning their nature and origin, it would be necessary to submit to stricter criticism than is generally done the phenomena attending their fall, together with their physical properties, mineralogical as well as chemical. We have no right to speak of the explosion of large bodies within our atmosphere, while the so-called fragments of them show no marks of any explosion; nor should we speak of superficial heating to fusion in our atmosphere, while masses of 50 lbs. weight were found, ten minutes after their fall, not warmer than any stone exposed to the sun's rays, while others fell on dry leaves without leaving on them any traces of combustion or heating. So I could point out several other erroneous views relative to the fall of meteorites, and fully refuted by the chemical and physical facts proved by the stones themselves, and about which my account of the Ohio fall in Silliman's Journal is to give some hints."

I have overcome, I believe, this difficulty by placing in the first period, viz. that of cosmical motion within the atmosphere, the formation of the crust by superficial fusion, and in the second period (that of telluric motion, or simple falling to the earth) the compensation between the internal and external temperatures. At all events, I may feel satisfied to see my own views to some extent corroborated by the independent assent of such a distinguished and competent observer as Prof. Laurence Smith.

Particles separated from the surface of meteorites, appearing perhaps to observers in the shape of sparks, may again be covered with a thinner crust, and belong to a later but still cosmical portion of the orbit, as B C (fig. 2).

It would be desirable to ascertain in new cases, and as far as possible in those of older date, what is the direction of the line C D with respect to the diurnal movement (west to east) of the

terrestrial surface, as the supposition of a tangential force adequate to the elevation required for experiments on free fall close to the earth's surface would prove inadmissible in the present case. At all events, observations on such fugitive phenomena require an uncommon amount of manifold circumspection.

Professor Laurence Smith concludes his above highly important memoir with the following propositions :—

1st. " The *luminous* phenomena attending the appearance of meteorites are not caused by *incandescence*, but rather by electricity, or some other agent.

2nd. " The sound comes not from the explosion of any solid body, but rather from *concussion* caused by its rapid movement through the atmosphere, partly also from electric discharge.

3rd. " Meteoric *showers* owe not their existence to fragments caused by the rupture of a single solid body, but to the division of *smaller* aërolites entering the atmosphere in groups.

4th. " The *black crust* is not of atmospheric origin, but is already formed in cosmical space, before the meteorites enter our atmosphere."

I think (says M. Haidinger) I have now given some explanation applicable to each of these four propositions ; some in the same sense (2nd and 3rd), the others (1st and 4th) in a somewhat different sense, without actually excluding mutual compromises. At all events, I would recommend the utmost accuracy in the observations of future meteoric falls, as well as in all investigations concerning those already known.

[To be continued.]

XLIV. *On the Silicates of Copper from Chile.* *By* Frederick Field, *F.R.S.E., M.R.I.A.**

ALTHOUGH I believe that crystallized silicate of copper has not yet been found in Chile, several varieties of this mineral exist in very large quantities, generally in amorphous masses, with various shades of colour, some of which are of considerable beauty. These silicates, owing to the great difference in their composition and to the entire absence of crystallization, have not excited the same amount of interest which has been attached to other species,—mineralogists supposing that they are not true minerals, but simply consist of oxide of copper in combination with silica in greater or less proportion, the varieties containing the most oxide being comparatively soft and friable, and the poorer kinds, having but little metal, being exceedingly hard and brittle, resembling in many respects masses of partially fused

* Communicated by the Author.

translucent glass. Some short account of those minerals which are found most frequently may perhaps not be entirely devoid of interest.

Green and Blue Silicates.—The Chilian miners frequently meet with veins of hard blue or green mineral, which they term *Llanca*, consisting, with the exception of small quantities of lime, alumina, and oxide of iron, entirely of oxide of copper, silicic acid, and water. The following is the composition of one of these llancas, which was found coating thin veins of suboxide of copper and the native metal, in the mines of Andacollo, Chile; the analysis was made by M. Domeyko:—

Oxide of copper . . .	29·50
Silica	52·20
Water	16·70
Alumina	1·20
	99·60

Another specimen of a pure green colour, analysed by the same chemist, yielded—

Oxide of copper . . .	12·00
Silica	75·90
Water	10·10
Alumina	2·00
	100·00

showing a very great difference in composition.

I obtained from a mine in the neighbourhood of Tambillos near Coquimbo, a considerable quantity of very fine silicate of copper, having a pure turquoise-blue colour, with little or no shade of green, perfectly amorphous and opake, and which appears, as the analysis will show, to have a far more definite composition than either of the samples quoted above:—

Oxide of copper . . .	39·50
Silica	28·21
Water	24·52
Oxide of iron	2·80
Alumina	4·97
	100·00

Regarding the alumina and oxide of iron as foreign to the mineral, we have in every 100 parts,—

Oxide of copper . . .	42·83
Silica	30·59
Water	26·58
	100·00

Silicate of copper, consisting of one equivalent of silicic acid,

one of water, and one of oxide of copper, would require the following numbers*:—

Oxide of copper . . .	41·23
Silica	30·94
Water	27·83
	100·00

The mineral may thus be regarded as nearly approaching in composition to $CuO\,SiO^2$, 3HO, and we have—

Dioptase	$CuO\,SiO^2$, HO.
Chrysocolla . . .	$CuO\,SiO^2$, 2HO.
Blue silicate . . .	$CuO\,SiO^2$, 3HO.

Black Silicate of Copper.—This mineral is of a dense black colour, of compact structure, conchoidal fracture with a glassy lustre, very much resembling obsidian. Before the blowpipe it does not change colour, and only fuses round the edges with difficulty. It gives off water when heated, and is easily attacked by hydrochloric acid. This silicate is found in some few mines in Chile, particularly those of the Higuera in the province of Coquimbo, and always in very narrow veins, which are generally found associated with the red oxide of copper, and the blue and green silicates. An analysis by Domeyko gave the following numbers:—

Oxide of copper . . .	50·10
Silica	28·20
Water	19·10
Oxide of iron	2·60
	100·00

In the year 1858 I published a short account of a double silicate of copper and manganese in the Chemical Gazette. The mineral, which in an impure state is found in considerable abundance, has a deep black colour, vitreous lustre, and is immediately decomposed by hydrochloric acid in the cold. A pure specimen yielded on analysis,—

Oxide of copper	24·71
Silica	18·90
Water	15·52
Oxide of iron	·23
Peroxide of manganese . . .	40·28
	99·64†

This mineral, as can be imagined, would prove highly valu-

* Silica is taken here at 30·20. Si = 14·2. 2O = 16.

† Chemical Gazette, vol. xvi. p. 105.

able could it be obtained of sufficient richness in copper to warrant its exportation to Europe, as, by the action of hydrochloric acid, large quantities of chlorine are evolved, which might be made available for the production of chloride of lime, and the residual solution of chloride of copper would yield the metal in a very pure state upon the introduction of metallic iron. Unfortunately, although the ore is plentiful, but small quantities are obtained having the composition given above, the average yield of copper scarcely exceeding 5 per cent.

The following Table may serve to illustrate the composition of some of the principal silicates of copper:—

Composition.	1.	2.	3.	4.	5.	6.	7.
Oxide of copper	50·00	44·94	29·50	12·00	50·10	39·50	24·71
Silica	38·70	34·83	52·20	75·90	28·20	28·21	18·90
Water	11·30	20·23	16·70	10·10	19·10	24·52	15·52
Oxide of iron	.....	.....	1·20	.....	1·46	2·80	·23
Alumina	.....	.....	.....	2·00		4·97	
Lime and magnesia							
Oxide of manganese	.....	.....	.....	.....	.....	.....	40·28
	100·00	100·00	99·60	100·00	98·86	100·00	99·64

(1) Dioptase.
(2) Chrysocolla
(3) Bluish-green amorphous silicate. Chile (Domeyko).
(4) Green silicate (Domeyko).
(5) Black silicate (Domeyko).
(6) Blue silicate. Chile (Field).
(7) Double silicate of copper and manganese. Chile (Field).

It may be mentioned that these silicates are, under certain circumstances, very advantageous to the smelter, especially when he has to operate upon highly ferruginous ores, by combining with the oxide of iron, and thus saving the sides of the furnace, which would otherwise be much injured. None of them contain either antimony or arsenic; and the copper therefore, after the necessary fusions, is exceedingly pure.

St. Mary's Hospital Medical School,
London, October 11, 1861.

XLV. *Note on the Readings of the Graduated Arc in Spectrum-Analysis, and Distortion of the Spectrum.* *By* J. M. Wilson, *B.A., Fellow of St. John's College, Cambridge, and Natural Philosophy Master in Rugby School**.

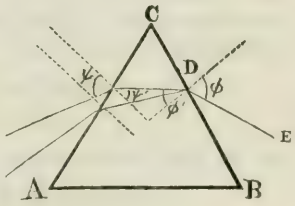
IN the ordinary apparatus for spectrum-analysis, the rays passing along the axis of a fixed telescope and incident on

* Communicated by the Author.

the prism are there refracted, and a portion of them pass down another telescope, and are brought to the vertical wire of its eye-piece. By moving either the telescope by which the rays are viewed, or the position of the prism, all the different lines of the spectrum can be seen in succession. In the first case the changes in the refrangibility of the rays and the requisite angular motion of the telescope are nearly proportional; in the second case, in which the prism is moved, it will be found that the angular changes in the position of the prism are by no means proportional to the changes in the indices of refraction of the rays corresponding to those positions.

The following investigation arose from a suggestion I made in the summer to Mr. Becker, that the readings on the graduated arc should be either the refractive indices of the lines corresponding, or should give the principal lines of the solar spectrum. Mr. Becker then requested me to examine how the scale might be so graduated.

1. Let A B C be a prism whose angle is α; D E a ray from the slit of the first telescope; ϕ, ϕ', ψ', ψ the angles of incidence and refraction at the first and second surfaces of the prism; μ_E the index of refraction for the line E; D the deviation for the line whose index is μ. Then



$$\sin\phi=\mu\sin\phi' \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$\sin\psi=\mu\sin\psi' \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$\phi'+\psi'=\alpha \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

$$\mathrm{D}=\phi+\psi-\alpha \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Let the angle between the telescopes, or the deviation, be fixed by the condition that the line X shall have a minimum deviation. Then

$$\phi=\psi,\ \phi'=\psi'=\frac{\alpha}{2},$$

and

$$\mathrm{D}=2\sin^{-1}\left(\mu_{\mathrm{X}}\sin\frac{\alpha}{2}\right)-\alpha.$$

The direction of DE being fixed, an angular change in the position of the prism is equivalent to a change in ϕ. It is required therefore to investigate a relation between μ and ϕ when the total deviation is fixed.

2. Differentiating the four equations given,

$$\cos\phi\frac{d\phi}{d\mu}=\mu\cos\phi'.\frac{d\phi'}{d\mu}+\sin\phi',$$

$$\cos\psi\frac{d\psi}{d\mu}=\mu\cos\psi'\frac{d\psi'}{d\mu}+\sin\psi',$$

$$\frac{d\phi'}{d\mu}+\frac{d\psi'}{d\mu}=0=\frac{d\phi}{d\mu}+\frac{d\psi}{d\mu}.$$

Eliminating $\frac{d\phi'}{d\mu}, \frac{d\psi}{d\mu}, \frac{d\psi'}{d\mu}$,

$$\frac{d\phi}{d\mu}=\frac{\sin\alpha}{\cos\phi\cos\psi'-\cos\psi\cos\phi'}.$$

This result indicates that when $\phi=\psi$ and $\psi'=\phi'$ nearly, the change in ϕ is very large for a small change in μ.

3. Using special values of μ and α, let the angle of the prism be 60°, and let $\mu_X=1{\cdot}6801330$. (The reason of selecting this value for μ_X will appear presently; and it cannot differ by any appreciable error from the value given by Müller for μ_E.) Then

$$\begin{aligned}D&=2\sin^{-1}.(\cdot8400665)-60^\circ\\&=54^\circ\ 17'\ 42'',\end{aligned}$$

and we have therefore the equations

$$\sin\phi=\mu\sin\phi', \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$\sin(114^\circ\ 17'\ 42''-\phi)=\mu\sin(60-\phi'). \quad . \quad . \quad (2)$$

Eliminating ϕ', we obtain

$$\frac{\sqrt{3}}{2}\sqrt{\mu^2-\sin^2\phi}=\sin(114^\circ\ 17'\ 42''-\phi)+\frac{1}{2}\sin\phi.$$

Now the sine and cosine of the $\angle$ 114° 17′ 42″ differ by ·5, and therefore the left-hand side of the equation becomes

$$\cdot9114369\,(\sin\phi+\cos\phi),$$

and therefore using logarithms,

$$\log(\mu^2-\sin^2\phi)=\cdot3451309+2\log\sin(45^\circ+\phi). \quad . \quad . \quad (A)$$

4. It will now be easy to calculate the value of μ for any values of ϕ.

When the deviation is a minimum,

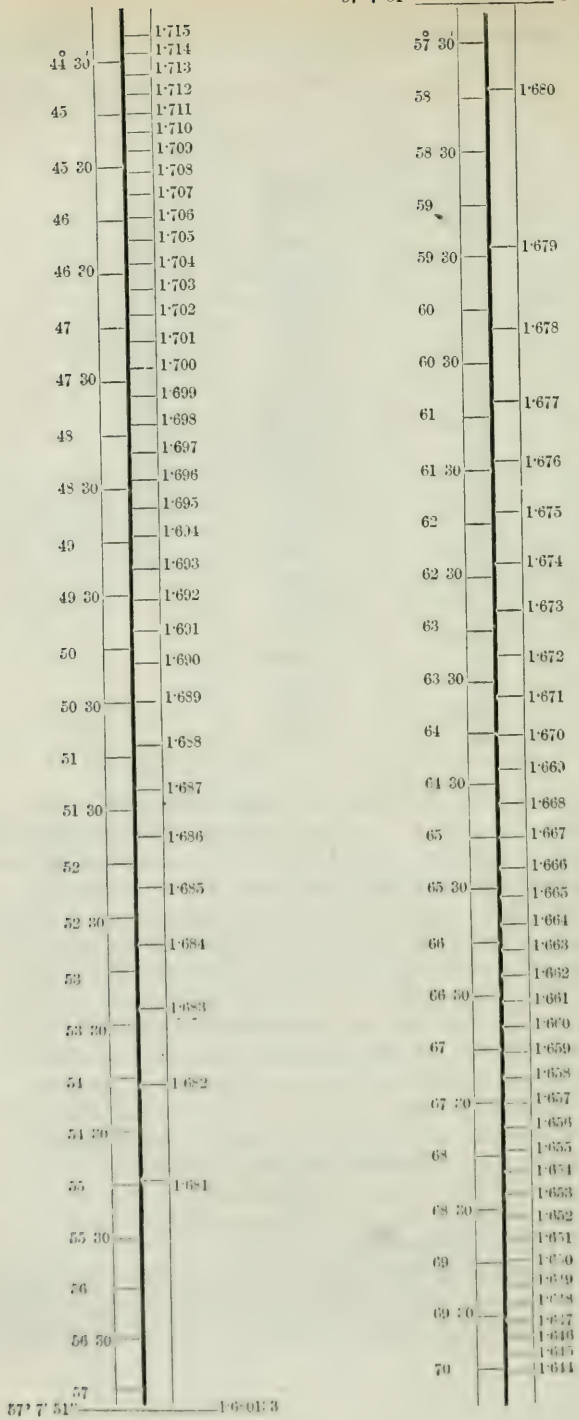
$$\phi=\frac{D}{2}+30^\circ, \text{ and } \phi=57^\circ\ 7'\ 51''.$$

I shall proceed to calculate the value of μ for all values of ϕ from 57° at intervals of half a degree, as far as is required by the limits of the spectrum, by means of the formula (A). The subjoined Table gives the results:—

ϕ.	μ.	Diff. of μ.
° ′ ″		
57 0	1·680148	
57 7 51	1·680133	
57 30	1·680125	
58	1·679993	132
58 30	1·679750	243
59	1·679398	352
59 30	1·678934	464
60	1·678361	573
60 30	1·677678	683
61	1·676884	794
61 30	1·675982	902
62	1·674969	1013
62 30	1·673847	1122
63	1·672623	1224
63 30	1·671259	1374
64	1·669824	1435
64 30	1·668277	1547
65	1·666596	1681
65 30	1·664796	1800
66	1·662935	1861
66 30	1·660941	
67	1·658840	
67 30		
68	1·654313	
68 30		
69	1·649359	
69 30		
70	1·643999	

From this Table it is easy to construct a curve which shall represent to the eye the relations of μ and ϕ; and either from the curve, or directly from the Table, to show to what extent the spectrum is distorted. The scale given on the following page is intended to illustrate this. The left column of the scale represents an arc graduated to half degrees, along which moves the index attached to the handle by which the prism is turned round. The degrees marked on the scale indicate the angle of incidence of the light on the first face of the prism. It might be convenient, however, to graduate this arc from the central line of minimum deviation (very near the line E) as zero. The right column of the scale gives the corresponding values of the refractive indices. It would be convenient to mark on one of the two scales the positions of the principal lines in the spectrum. This might be done either by experiment, or from the value of the refractive indices into bisulphide of carbon of the fixed lines, if they have been determined.

This distortion of the spectrum, or apparent exaggeration of its central portions, may be made evident to the eye by turning the prism slowly round with a uniform motion by moving the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57° 7′ 51″
1·6·01·3
57° 30′
58
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index from one end of the scale to the other, while the eye is applied to the telescope. The spectrum will then be seen to move with varying velocity. It will seem at first to move fast, and then gradually to diminish in velocity till the middle of the scale is reached, when it will be for the moment stationary, and then begin to move slowly in the same direction as before and with ever-increasing velocity till the end of the scale is reached. This also accounts for a fact which every one who has used the instrument must have observed, viz. the great preponderance of green in the spectrum thrown by the sun or common gaslight. It is obvious that the knowledge of these results gives a method of magnifying any portion of the spectrum, and of determining its limits with greater accuracy.

XLVI. *On the most advantageous Form of Magnets.* *By* Dr. Lamont*.

[With a Plate.]

THE further the science of magnetism advances in its development, the more important becomes the decision of the question *what form should be given to the magnets in order to obtain the most advantageous effect.* If we at present confine ourselves to those magnets which have been employed in the investigation of the magnetism of the earth, we find that by some observers very acute sharp-pointed needles, by others flat prismatic needles, have been pronounced to be the best adapted to the purpose; solid or hollow cylinders also have been recommended. I am not aware, however, that experiments of a discriminating character have as yet been carried out; nor, so far as I know, have even the principles been established according to which the preference of one form over the others is to be determined. Nevertheless, as regards the latter point, a nearer consideration will show that scarcely an uncertainty or difference of opinion can exist, as there are in general only *three* subjects of observation which come into consideration in magnets, and it cannot be doubtful in what relations to the result these determinations stand.

The three determinations here referred to are—the *magnetic moment*, the *weight* or the *mass*, and the *moment of inertia*; and that form is to be recognized as the most advantageous, in which are united the greatest possible magnetic moment, with the smallest possible mass, and the smallest possible moment of inertia.

The direct way to decide upon the most advantageous form of

* Translated from Poggendorff's *Annalen*, vol. cxiii. pp. 239–249. Communicated by the Astronomer Royal.

the magnets, would consist in procuring hardened steel bars of different forms, magnetizing them to saturation, and investigating by measurement for every form the above-mentioned determinations. By this method I have instituted different experiments, but have given to them no great extension, because I have found another way which attains the desired end more simply and more surely.

A magnet is composed of *magnetic molecules*. If the molecules were separated, it would appear that every molecule forms a small magnet with a determinate quantity of positive and negative magnetism; and this is what I denote by *independent magnetism*. As soon as the molecules are put together, each induces in the rest new magnetism, and to the independent magnetism of each molecule there is added a more or less considerable quantity of *induced magnetism*, according to the situation which the molecule occupies in the magnet.

The whole effect of a magnet is regulated by the *independent* and *induced* magnetism of the molecules.

A magnet is *then* magnetized to saturation when every molecule possesses the *greatest possible independent* magnetism; from which of course it follows that in a bar magnetized to saturation all the molecules have *equal* independent magnetism.

Now if we introduce a soft iron core of tolerable length into a very long spiral, through which a galvanic current passes, it is known that the same magnetizing force is exerted upon every molecule of the iron core; that is to say, equal independent magnetism is imparted to each molecule, and the mutual induction of the molecules comes then into operation as in the molecules of a magnet. From this it results that the distribution of the magnetism in an iron core placed within a long spiral, and that in a magnet which is magnetized to saturation, will be *the same*; and the laws under the limitations above-mentioned can be determined quite as well by iron cores as by magnets. But by substitution of iron cores, the great advantage is obtained, that the investigation is not only more easily executed, but also the disturbing influences which arise from the unequal or disproportionate hardness of different bars, and the consequent uncertainty whether, in the magnetizing, the point of saturation* is really reached, totally disappear.

* If a needle is rubbed with a pair of magnetic bars which are somewhat larger than the needle itself, and this rubbing is continued till the needle no longer receives additional magnetism, it is said to be "magnetized to saturation," although it is not proved whether a far greater magnetism might not be imparted by more powerful instrumental means. On the magnitude of the means which is required to communicate the maximum of force, no satisfactory investigations have hitherto been instituted; and

Fig. 6.

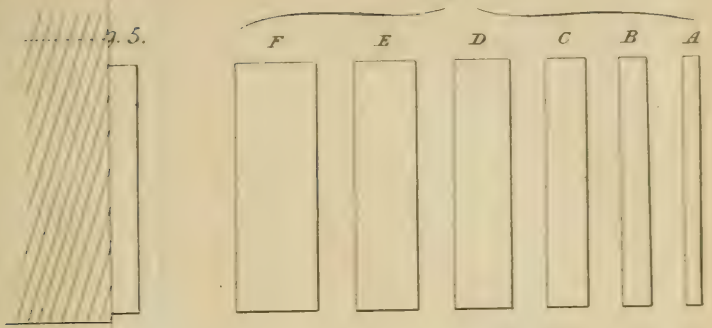


Fig. 5.

Fig. 8.

Fig. 11.

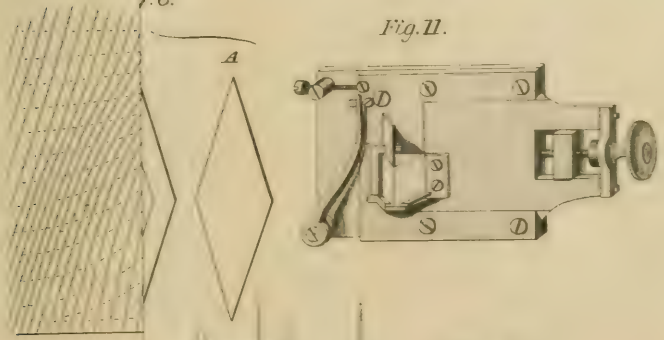
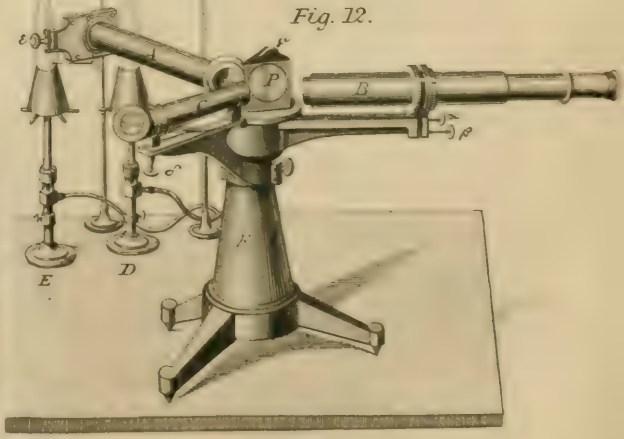


Fig. 12.



J. Bastire sc.

The principles above stated are here only mentioned *en passant*, as they have been already on an earlier occasion * stated, at least in outline, and will be hereafter more fully explained in a more detailed memoir. It has been already stated above, that the more or less advantageous form of a magnet is to be decided by the proportion of the magnetic moment to the mass, and to the moment of inertia: now, as to the last, it only comes into consideration in the oscillations, and it is of more trifling significance, on which account we will first investigate the proportion of the magnetic moment to the mass.

1*st Series of Experiments*.—In order to ascertain the dependence of the magnetism upon the diameter, I caused to be prepared four pieces of iron (Plate VI. fig. 4) of equal length $=43'''\cdot2$ (Paris measure), and equal weight, but different transverse sections; the sections were,—

Of A, an equilateral triangle; length of one side $= 7'''\cdot5$.
Of B, a circle; diameter $= 5'''\cdot7$.
Of C, a square; length of one side $= 5'''\cdot3$.
Of D, a parallelogram; sides $= 6'''\cdot0$ and $4'''\cdot1$.
Of E, a parallelogram; sides $= 12'''\cdot4$ and $2'''\cdot1$.

In a long spiral of 212 turns, these pieces of iron, inserted as cores, gave the following magnetic moments (reduced to equal strength of current):—

	Magnetic moment.	Mass.	Proportion.
A . . .	7·255	1·00	7·255
B . . .	6·806	0·99	6·875
C . . .	7·300	1·14	6·404
D . . .	6·952	1·05	6·621
E . . .	8·248	1·13	7·299

---

how little foundation there is for the ordinary opinions may be seen from the following statement.

In the mechanical workshop of the observatory of this city are two magnetizing apparatus, of which the one consists of two 25-pound bars, and the other is an electro-magnetic apparatus of great energy. Amongst the experiments which were carried out to prove the relation of the two apparatus appears the following case.

Two prismatic magnets, length $56'''\cdot0$ and $56'''\cdot6$, breadth $6'''\cdot8$ and $4'''\cdot9$, thickness $1'''\cdot5$ and $1'''\cdot0$, perfectly hard, were magnetized with the 25-pound bars, and the magnetic moment was determined by means of the deviation, whereby I obtained—

Greater Magnet, deviation .....116·3 scale-divisions.
Smaller Magnet, deviation ..... 81·7 scale-divisions.

Afterwards, when the two needles had been magnetized by means of the electro-magnetic apparatus, there resulted—

Greater Magnet, deviation .....177·8 scale-divisions.
Smaller Magnet, deviation .....112·4 scale-divisions.

From this we see that the magnetizing by the 25-pound bars, in regard to the degree of saturation, was deficient, in the greater magnet by somewhat more, and in the smaller by somewhat less, than a third part.

* *Jahresbericht der Münchener Sternwarte für* 1854, p. 27.

The mass is here, as well as in the following series of experiments, determined by means of the balance, not deduced from the dimensions above given, which are only approximate.

The most disadvantageous forms are the prism with square section and the cylinder, in which the mass is collected as much as possible about the axis of the figure, while, on the other hand, the greater widening of the mass in the other forms apppears to possess considerable advantage.

2nd Series of Experiments.—Twelve equal laminæ of iron plate (fig. 5), length $43'''\cdot2$, breadth $5'''\cdot3$, thickness $0'''\cdot4$, were so managed that first a single one, then two, three, &c. laid together, or rather joined together, were brought into the abovementioned spiral. When the twelve laminæ were laid together, they formed a prism of equal magnitude with C in the first series of experiments, and had a weight of 94·8 grms. The results were,—

	Magnetic moment.	Proportion to the mass.
1 lamina . . .	3·53	3·53
2 laminæ . . .	4·11	2·05
3 „ . . .	4·36	1·45
4 „ . . .	4·65	1·16
5 „ . . .	4·94	0·99
6 „ . . .	5·15	0·86
7 „ . . .	5·39	0·77
8 „ . . .	5·61	0·70
9 „ . . .	5·83	0·65
10 „ . . .	6·05	0·60
11 „ . . .	6·27	0·57
12 „ . . .	6·44	0·54

Here is shown in a striking manner how disadvantageous it is to increase the thickness.

As a deduction from the results given above, 14·4 parallelograms would, according to the weight, be equal to the prism C (Series of Experiments 1), and the whole magnetism of the same would have amounted to 6·874; but a double comparison gave 7·194—without doubt a consequence of this, that the parallelograms had been covered in the heating with tinder [Zunder].

3rd Series of Experiments.—Six parallelograms (fig. 6) of $45'''\cdot6$ length, $0'''\cdot3$ thickness, and breadths $2'''\cdot3$, $4'''\cdot6$, $6'''\cdot8$, $9'''\cdot1$, $11'''\cdot4$, $13'''\cdot7$, were cut out of an iron plate, and after they had been carefully heated were brought into the abovementioned spiral; the result was as follows:—

	Magnetic moment.	Mass.	Proportion to the mass.
A . . .	2·69	2·8	0·961
B . . .	4·05	5·8	0·699
C . . .	5·04	9·0	0·560
D . . .	5·77	11·7	0·493
E . . .	6·52	14·3	0·454
F . . .	7·12	16·7	0·425

From this it is deducible that the augmentation of the breadth is also to be considered as disadvantageous, but in a more trifling proportion than we have found that of the thickness in the second series of experiments.

4th Series of Experiments.—Four needles (fig. 7) contracting from the middle to sharp points at the two ends (rhomboids) were cut out of an iron plate. They had all the same length $=59'''\cdot6$; the breadths in the middle were very nearly in the proportion of 1, 2, 3, 4, and amounted in the broadest needle to $19'''\cdot5$. The observation gave the following numbers:—

	Magnetic moment.	Mass.	Proportion to the mass.
A . . .	4·304	4·95	0·870
B . . .	5·313	9·84	0·539
C . . .	5·944	14·45	0·412
D . . .	6·595	19·45	0·339

It appears hereby that the proportion of the magnetism to the weight is the more advantageous the more sharply the needles are pointed, that is, the smaller the breadth is in the middle.

5th Series of Experiments.—Three equal needles (fig. 8) were made in form similar to those of the fourth series; length $46'''\cdot0$, breadth in the middle $13'''\,3$; from two of them a part was taken out of the middle, so that they had the appearance of perforated rhomboids, and the part cut out was similar to the whole figure. The magnitude of the part cut out amounted in B to one-third, and in C to two-thirds of the whole figure. The observation gave—

	Magnetic moment.	Mass.	Proportion to the mass.
A . . .	3·46	1·02	3·39
B . . .	3·47	0·85	4·08
C . . .	3·17	0·52	6·04

It is therefore very advantageous to take out a part of the mass in the middle.

6th Series of Experiments.—In the fourth and fifth series of experiments the needle contracted to a point from the middle towards the two ends; in the present series of experiments it is to be ascertained what difference depends on the circumstance whether the breadth begins to diminish directly from the middle or nearer to the ends. For this purpose flat pieces of steel were employed of $43'''\cdot1$ length, $1'''\cdot0$ thickness and $10'''\cdot0$ breadth (in the middle), whose figure is represented in fig. 9; the part $a\,b$ amounted in B to a sixth, in C to a third, and in D to a half of the length. The results were—

	Magnetic moment.	Mass.	Proportion to the mass.
A . . .	44·6	37·2	1·20
B . . .	34·3	28·8	1·19
C . . .	27·7	23·6	1·17
D . . .	23·6	18·0	1·32

This series of observations is not very decisive; nevertheless it shows distinctly that the pointing of the ends of the magnets is not advantageous, except when the diminution of the breadth begins from the middle. A flat needle contracting its breadth from the middle to a point is, by the above measures, more advantageous by one-tenth than one of the form of a parallelogram; from other far more decisive series of experiments I have found a somewhat greater proportion, *i. e.* one-eighth.

7th Series of Experiments.—It is known that magnetism shows its strength in corners and points, and it appeared proper to investigate what result would be obtained if a magnet had several points. With this view, three parallelograms of $47'''\cdot0$ length, $9'''\cdot0$ breadth, $0'''\cdot4$ thickness, were cut out of a plate of iron, and triangular notches were cut out of the ends, so that one piece received two, the other three points at each end, whilst in the third piece no cut was made. The form of the pieces is seen in fig. 10; the depth of the cuts amounted to a fourth of the length. The observation gave—

	Magnetic moment.	Mass.	Proportion to the mass.
A . . .	5·075	1·00	4·659
B . . .	4·908	1·10	4·462
C . . .	6·005	1·41	4·259

According to this it is advantageous to cut notches in the ends of flat magnets, and the proportion increases with the number of notches.

As a consequence of the determination given in the sixth series of experiments, the proportion-number would be 4·79 for a needle contracting to a point from the middle; it is not improbable that by increase of the number of notches this proportion could be exceeded, nevertheless the figure recommended itself, as to what is here in question, so little in other respects, that it will scarcely find practical application.

From the preceding determinations it results—

(1) That *narrower* magnets are more advantageous than *broader*.

(2) That *thinner* magnets are more advantageous than *thicker*.

(3) That consequently the most advantageous form is that in which breadth and thickness disappear, and the magnet is

transformed into a mathematical line, *i. e.* into a so-called *linear magnet*.

The most advantageous form of a magnet, so far as the proportion of the magnetism to the weight is considered, is therefore an *imaginary* one; practically, however, there are two forms which appear advantageous, namely the *flat, contracting to a point from the middle*, and the *flat prismatic*: and indeed in the former form the proportion of the magnetism to the weight is more advantageous by one-eighth part than in the latter; so that it must always hold as a rule that the thickness and breadth must be as far diminished as the other necessary conditions permit.

We should still have to investigate in what proportion in the above-mentioned forms the magnetism stands to the moment of inertia; but I consider it superfluous to annex here the tabular exhibitions relative to this, since without such it is easy to see that the form which we have pronounced as disadvantageous in reference to the weight, must also be disadvantageous as regards the moment of inertia. But as respects the flat form contracting to a point from the middle, and the flat prismatic form, which have been noted above as the only appropriate forms, the weights are, with equal length, and equal breadth in the middle, as 1 to 2, and the moments of inertia as 1 to 3·75, so that the form contracting to a point must be recognized as by far the best.

In regard to the preceding investigation, it ought yet to be mentioned that it must prove always too much dependent on circumstantial details, and too little satisfactory, as long as we are not in a position to lay down the laws of the distribution of the magnetism and of the dependence of the magnetic moment upon the dimensions. In this latter point of view the labours hitherto employed have had only very trifling success. From numerous observations which I have made with the prismatic bars, it results that with equal thickness the magnetic moments are in the proportion of the square roots of the thickness; nevertheless this law only obtains for greater transverse sections, and is perfectly unavailable for smaller dimensions. I have now made substitutions in the formula

$$\sqrt{\frac{ax+b}{x+c}}\,x,$$

where x is the variable dimension, and a, b, c constants; and I find that it very accurately corresponds with observation in small as in great dimensions. Even when laminæ are laid together, this formula represents very well the result, as will be proved by the following Table, in which the second series of experi-

ments is calculated by the formula

$$\sqrt{\frac{12{\cdot}80+2{\cdot}46\,n}{n+0{\cdot}218}}\,n.$$

Number of laminæ $=n$.	Magnetic moment. Observed.	Calculated.	Difference.
1 . . .	3·53	3·54	+0·01
2 . . .	4·11	4·00	−0·11
3 . . .	4·36	4·34	−0·02
4 . . .	4·65	4·63	−0·02
5 . . .	4·94	4·90	−0·04
6 . . .	5·15	5·16	+0·01
7 . . .	5·39	5·40	+0·01
8 . . .	5·61	5·62	+0·01
9 . . .	5·83	5·84	+0·01
10 . . .	6·05	6·05	0·00
11 . . .	6·27	6·25	−0·02
12 . . .	6·44	6·45	+0·01

A practical inference results from the preceding investigation, which I believe deserves to be carefully considered on the part of those who concern themselves with the manufacture of magnetic instruments. A freely moveable magnet is to be employed with advantage only so far as the magnetic moment is as large as possible in proportion to the weight. But the more the transverse size is augmented, the greater is the departure from the fulfilment of this condition, and consequently the use of massive magnetic bars must be pronounced inadmissible. There is only one means of obtaining great magnetic strength with trifling weight; namely, by firmly connecting several thin and flat magnets near or upon one another in one system without their touching each other. Many years ago I began in magnetic variation-instruments, later also in magnetic theodolites, to unite several magnets; and at present I use universally systems of three laminæ, which are laid upon each other and held separated in the middle by small pieces of brass of about the thickness of three-quarters of a line. Also in ships' compasses several needles near each other are at present continually used with the best result. Hollow cylindrical magnets, to which some artists have given a great preference in regard to strength and lightness, remain, as can be proved even from theoretical considerations, very far behind in comparison with a single flat needle; and with this agree also the experiments which I have made.

XLVII. *Remarks on Radiation and Absorption.*

To Sir John F. W. Herschel, Bart. &c. &c. &c.

Dear Sir John,

I AM anxious to address this note to you upon a subject which you have in great part made your own, because I fear that neither in my book upon the Alps, nor in my recently published papers, have I made due reference to your estimable researches on Solar Radiation. I have been for some time experimenting on the permeability of our atmosphere to radiant heat, and have arrived at the conclusion that true air, that is to say, the mixture of oxygen and nitrogen which forms the body of our atmosphere, is, as regards the transmission of radiant heat, a practical vacuum. The results from which the opacity of air has been inferred are all to be ascribed to the extraneous matters diffused in the atmosphere, and mainly to the aqueous vapour. The negative results recently obtained by that eminent experimenter, Professor Magnus of Berlin, have induced me to reinvestigate this point; and the experiments which I have made not only establish the action of aqueous vapour, but prove this action to be comparatively enormous. Here is a typical case:—On the 10th of this month I found the absorptive action of the common air of our laboratory to be made up of three components, the first of which, due to the pure air, was represented in magnitude by the number 1; the second, due to the transparent aqueous vapour, was represented by the number 40; while the third, due to the effluvia of the locality and the carbonic acid of the air, was represented by the number 27. The total action of its foreign constituents on the day in question was certainly sixty-seven times that of the atmosphere itself; while the aqueous vapour alone exerted an action at least forty times that of the air.

I have also to communicate to you some results of lunar radiation which connect themselves with your speculations. On Friday the 18th of this month, I made a series of observations on the moon from the roof of the Royal Institution. From six concurrent experiments, I was compelled to infer that my thermo-electric pile lost more heat when presented to the moon than when turned to any other portion of the heavens of the same altitude. The effect was equivalent to a radiation of *cold* from our satellite. I was quite unprepared for this result, which, however, you will at once perceive, may be an immediate consequence of the moon's *heat*. On the evening in question a faint halo which surrounded the moon, and which was only visible when sought for, showed that a small quantity of precipitated vapour was afloat in the atmosphere. Such precipitated particles, in virtue of their multitudinous reflexions, constitute a

powerful screen to intercept the terrestrial rays, and any agency that removes them and establishes the optical continuity of the atmosphere must assist the transmission of terrestrial heat*. I think it may be affirmed that no sensible quantity of the obscure heat of the moon, which, when she is full, probably constitutes a large proportion of the total heat emitted in the direction of the earth, reaches us. This heat is entirely absorbed in our atmosphere; and on the evening in question it was in part applied to evaporate the precipitated particles, hence to augment the transparency of the air round the moon, and thus to open a door in that direction for the escape of heat from the face of my pile. The instrument, I may remark, was furnished with a conical reflector, the angular area of which was very many times that of the moon itself.

I remain,
Yours very faithfully,
October 21, 1861. JOHN TYNDALL.

XLVIII. *On a Generalization of a Theorem of* Cauchy *on Arrangements. By* J. J. SYLVESTER, *M.A., F.R.S., Professor of Mathematics at the Royal Military Academy, Woolwich*†.

IN a paper "On the Theory of Determinants" in the Philosophical Magazine for March in this year, Mr. Cayley has referred and added to a theorem of Cauchy deduced from the latter's method of *arrangements*, viz. that if we resolve an integer n in every possible way into parts, to wit α parts of a, β parts of $b, \ldots$ of l, (a, b, $c \ldots l$ being all distinct integers), then

$$\Sigma \frac{1}{\Pi\alpha a^{\alpha}\,\Pi\beta b^{\beta}\ldots\Pi\lambda l^{\lambda}} = 1.$$

Now both Cauchy's theorem and Mr. Cayley's addition to it (which essentially consists in the observation, that if before the numerator 1 in the above quantity under the sign of summation we write $(-)^{\alpha+\beta+\cdots+\lambda}$, the sum becomes zero) are no more than particular cases of the following theorem: viz. that if

* I was going to add "into space;" but the expression might lead to misapprehension. My experiments indicate that the absorption of water is a *molecular* phenomenon. If we suppose the aqueous vapour of the atmosphere to be condensed to a liquid shell enveloping the earth, the experiments of Melloni would lead us to conclude that such a shell would completely intercept the obscure terrestrial rays. And if the vapour be equally energetic, our atmosphere would prevent the *direct* transmission of the obscure heat of the earth into space. On this point, however, I wish to make some further observations.

† Communicated by the Author.

instead of 1 we write $\rho^{\alpha+\beta+\ldots+\lambda}$ in the numerator of the quantity under the sign of summation (ρ being any quantity whatever), the sum becomes expressible as a known function of ρ. Nothing can be easier than the proof.

Let the $\alpha, \beta, \gamma, \ldots \lambda$ in the preceding statement be supposed subject to the further condition that their sum is r; then for any assigned value of r (a positive integer) it is easy to see that the sum of the terms within the sign of summation in Cauchy's theorem is

$$\mathrm{S}\left(\frac{1}{x_1\, x_2 \ldots x_r} \cdot \frac{\rho^r}{\Pi(r)}\right),$$

where $x_1, x_2, \ldots x_r$ mean *every* system of values of $x_1, x_2, \ldots x_r$ (permutations *admitted*) which satisfy the equation

$$x_1 + x_2 + \ldots + x_r = n.$$

(It should here be observed that $\alpha, \beta, \gamma, \ldots \lambda$; $a, b, c, \ldots l$ are the systems which satisfy $\alpha a + \beta b + \gamma c + \ldots + \lambda l = n$, permutations being *excluded*; that is to say, if, for example, α, β, γ should happen to be equal for any partition of n, the values α, a; α, b; α, c would figure only *once*, and not *six* times, among the systems included under the sign of Σ.) Hence then we see that

$$\Sigma \frac{\rho^{\alpha+\beta+\gamma+\ldots+\lambda}}{\Pi\alpha \,.\, a^\alpha \;\; \Pi\beta \,.\, b^\beta \ldots \Pi\lambda \,.\, l^\lambda} = \Sigma_{r=\infty}^{r=1} \mathrm{S}_r \frac{\rho^r}{\Pi(r)} *.$$

where S_r is the coefficient of t^n in $\left(\frac{t}{1} + \frac{t^2}{2} + \frac{t^3}{3} + \text{\&c. } ad\ infin.\right)^r$, *i. e.* in $\left(\log\left(\frac{1}{1-t}\right)\right)^r$; and the total sum designated by Σ will be consequently the coefficient of t^n in

$$\log\left(\frac{1}{1-t}\right)\rho + \left(\log\frac{1}{1-t}\right)^2 \frac{\rho^2}{1\,.\,2} + \text{\&c.},$$

i. e. in $e^{\rho \log\left(\frac{1}{1-t}\right)}$, *i. e.* in $\left(\frac{1}{1-t}\right)^\rho$.

---

* For if we take a system of values satisfying the above equation, consisting of α equal values a, β equal values $b \ldots \lambda$ equal values l, such a system will give rise in $\Sigma \frac{1}{x_1\, x_2 \ldots x_r}$ to $\frac{\pi(r)}{(\pi\alpha)\,(\pi\beta)\ldots(\pi\lambda)}$ repetitions of the term $\frac{1}{a^\alpha \,.\, b^\beta \ldots l^\lambda}$, and consequently in $\Sigma \frac{1}{x_1\, x_2 \ldots x_r} \cdot \frac{1}{\pi r}$ to a total value $\frac{1}{(\pi\alpha)a^\alpha\,(\pi\beta)b^\beta \ldots (\pi\lambda)l^\lambda}$, condensed into a single term in Cauchy's theorem.

Thus if $\rho=1$, we have Cauchy's theorem, viz. $\Sigma=1$;

„ $\rho=-1$, „ Cayley's theorem, viz. $\Sigma=0$*;

and in general for any value of ρ,

$$\Sigma=\frac{\rho(\rho+1)\ldots(\rho+n-1)}{1.2\ldots n}\dagger.$$

In this theorem is in fact included another, viz. that if

$$\alpha a+\beta b+\ldots+\lambda l=n \text{ and } \alpha+\beta+\ldots+\lambda=r$$

(permutations *not* admissible), then

$$\Sigma\frac{\Pi n}{\Pi\alpha\,.\,a^{\alpha}.\,\Pi\beta\,.\,b^{\beta}\ldots\Pi\lambda\,.\,c^{\lambda}}$$

is equal to the coefficient of ρ^{r-1} in

$$(\rho+1)(\rho+2)\ldots(\rho+n-1).$$

This coefficient is accordingly (to return to Cauchy's theory of arrangements) the number of substitutions of n elements capable of being expressed by the product of r cyclical substitutions. As, for instance, the number of substitutions of four elements a, b, c, d capable of expression by the product of two cyclical substitutions

* Provided, however, that n exceeds 1, a limitation accidentally omitted in Mr. Cayley's paper; and so in general

$$\Sigma\frac{(-\rho)^{\alpha+\beta+\ldots+\lambda}}{\Pi\alpha\,.\,a^{\alpha}\ldots\Pi\lambda\,.\,l^{\lambda}}=0,$$

ρ being *any* positive integer provided n is greater than ρ.

† If $\rho=\frac{1}{2}$, we obtain

$$\Sigma=\frac{1.3.5\ldots(2n-1)}{2.4.6\ldots 2n};$$

from which it is easy to infer that the number of substitutions of $2n$ things representable by the product of cyclical substitutions, all of an even order, is $(1.3.5\ldots(2n-1))^2$. If $\rho=-\frac{1}{2}$, we obtain

$$\Sigma=\frac{1.1.3\ldots(2n-1)}{2.4.6\ldots(2n)},$$

combining which with the preceding result, it is easy to infer that the number of substitutions of $2n$ things representable by the product of an odd number of cyclical substitutions, all of an even order, is to the number of such representable by the product of an even number of cyclical substitutions, all of an even order, in the ratio of n to $(n-1)$. The former of these two theorems is intimately allied with Mr. Cayley's celebrated theorem on "skew," or what, for good reasons hereafter to be alleged, I should prefer to call *polar* determinants, viz. that every such of the $2n$th order is the square of a *Pfaffian*. A Pfaffian is in fact a sum of quantities typifiable completely, both as to sign and magnitude, by a duadic *syntheme* of $2n$ elements, the number of which is readily seen to be $1.3.5\ldots(2n-1)$. I believe I shall soon be in a condition to announce a remarkable extension of this theory to embrace the case of Polar *Commutants* and *Hyperpfaffians*.

ought to be the coefficient of λ in $(\lambda+1)(\lambda+2)(\lambda+3)$, *i. e.* 11, which is right; for the number of substitutions of the form $(a, b)\ (c, d)$ will be 3, and of the form $(a, b, c)\ (d)$ 8. In conclusion, I may notice that by an obvious deduction from this last theorem, we are led to the well-known one in the theory of numbers, that every coefficient in the development of

$$\Sigma(\rho+1)(\rho+2)\ldots(\rho+n-1),$$

except the first and last, and the sum of these two, is divisible by n when n is a prime number; and indeed we can actually express by aid of it the quotient of every intermediate coefficient divided by n as the sum of separate integer terms free from the sign of addition.

K, Woolwich Common,
October 10, 1861.

Postscript. By an extension of the method of generating functions contained in the text above, it may easily be seen that the number* of substitutions of n letters represented by the products of r cyclical substitutions, where the number of letters of each cycle leaves a given residue e in respect to a given *modulus* μ, may be made to depend on the solution of the equation in differences

$$u_n - u_{n+\mu} = \frac{\rho}{n-e}\, u_{n-e}.$$

The case where $e=1$ is deserving of particular notice.

It may be shown by means of the above equation in differences, that the number of substitutions of n letters formed by r cycles each of the form $\mu\mathrm{K}+1$ (μ being constant), say $\phi(n, r, \mu, 1)$, where $\frac{n-r}{\mu}$ is necessarily an integer, may be found by taking in every possible way $\frac{n-r}{\mu}$ *distinct* groups of μ consecutive terms of the series $1, 2, 3, \ldots (n-1)$; the sum of the products of every such combination of groups is the value required. For example, if

$$n=8, \quad r=3, \quad \mu=2,$$

---

* For this number, divided by $\Pi(n)$, is the coefficient of x^n in

$$\frac{1}{\Pi r}\left(\int_0^x \frac{dx\, x^{e-1}}{1-x}\right)^r, \text{ say } \frac{1}{\Pi r}(\phi x)^r,$$

and therefore of $x^n\rho^r$ in $e^{\rho\phi x}$, say $\psi(x, \rho)$, and therefore (since $\frac{d\psi}{dx} = \frac{x^{e-1}}{1-x^m}$ and ψ may be put under the form $\Sigma \frac{u_n}{n} x^n$) of ρ^r in $\frac{u_n}{n}$, where u_n is defined as in the text.

$$\phi(8,3,2,1)=1.2.3.4.5.6+1.2.3.5.6.7+1.2.3.5.6.8$$
$$+1.2.3.6.7.8+2.3.4.5.6.7+2.3.4.6.7.8$$
$$+3.4.5.6.7.8.$$

And as a corollary, since it may easily be seen that $\phi(n, r, \mu, e)$ is always divisible by n when n is a prime and $\mu r+e<n$, it follows that the sum of all the possible products of (any given number) i distinct groups of a given number r of consecutive terms of the series $1, 2, 3, \ldots (n-1)$ will be divisible by n when n is a prime and $ir<n-1$*. When $r=1$, this theorem becomes identical with Wilson's, already referred to.

Finally, it may be noticed that the number of substitutions of n letters formed by *any* number of cycles, all of an *odd* order, will be the coefficient of x^n in $\left(\frac{1+x}{1-x}\right)^{\frac{1}{2}}$, *i. e.* $\big(1.3.5\ldots(n-1)\big)^2$ (the same as the number that can be formed with cycles all of an *even* order) when n is even, and $\big(1.3.5\ldots(n-2)\big)^2 n$ when n is odd †.

XLIX. *Notes on the Hexahedron inscribed in a Sphere.*

By Charles W. Merrifield, *Esq.*‡

1. Its six planes pass, in general, four by four through three points.

There is exception, as a singular case, where the intersections of two of the three pairs of opposite planes are parallel. In this case the intersection of the third pair is perpendicular to the parallels, and the inclination of the two planes equal, but opposite.

Let us consider the intersection of the sphere with any pair

* For instance, making $n=7$, $r=2$, $i=2$,

$1.2.3.4+1.2.4.5+1.2.5.6+2.3.4.5+2.3.5.6+3.4.5.6=784$

and is *divisible by* 7.

† By taking $\mu=2$ in the general theorem, it is an easy inference that if we write

$$(\tan^{-1}x)^r=x^r-\frac{\Lambda_2 x^{r+2}}{(r+1)(r+2)}+\frac{\Lambda_4 x^{r+4}}{(r+1)(r+2)(r+3)(r+4)}\mp\&c.,$$

Λ_{2i} will be the sum of all the products of $2i$ integers comprised between 1 and $r+2i-1$ that can be formed with combinations of i distinct pairs of consecutive integers; thus (*e. g.*) the coefficient of x^{2m} in $(\tan^{-1}x)^2$ ought to be

$$\frac{1}{m}\left(1+\frac{1}{3}+\frac{1}{5}+\ldots+\frac{1}{2m-1}\right),$$

which may be easily verified.

‡ Communicated by the Author.

of opposite planes separately. A cone can in general be drawn through the two circles of intersection, and the truncated portion of any four-sided pyramid inscribed in this cone will give an inscribed hexahedron in the sphere. Moreover, if a quadrangle be inscribed in one of the circles, as ABCD, and a plane be drawn through AB cutting the other circle in *ab*, and then two more planes be drawn through the points *a*AD and *b*BC cutting the second circle in *cd* respectively, it will be seen that the quadrangle CD*cd* will not be plane unless—

(α) The plane AB*ab*, and hence the three others, pass through the vertex of the cone, or

(β) Either pair of lines AB and CD or AC and BD be parallel to one another and perpendicular to the common diametral plane of the cone and sphere.

(α) is the general case, and the hexahedron is a frustum of a quadrangular pyramid. By considering in like manner the other two pairs of planes, it will be seen that the hexahedron is the common frustum of three four-sided pyramids.

(β) is the singular case of a four-sided prism, the two sections being equally inclined, but in opposite directions. Note: that this prism will not in general be inscribed in a right cylinder.

2. In the general case it obviously follows that the hexahedron has six diagonal planes, passing two by two through the three vertices of the pyramids.

3. The four diagonal lines of the hexahedron intersect in a point. This follows, in the general case, from their lying two by two on the six diagonal planes, and it is easily seen in the singular case.

4. Hence in both cases there are six diagonal planes, all intersecting in a point.

5. This point is the spherical pole to the plane passing through the three vertices. In the singular case the polar plane passes through the parallel lines of intersection. This property may be deduced from the similar one of plane quadrilaterals inscribed in a circle.

6. Since the sections of the cone by the planes ABCD, *abcd* are subcontrary, the corresponding angles A*a*, B*b*, C*c*, &c. are equal each to each. Hence the sum of the opposite angles BAD and *bcd* is equal to two right angles, and so of similarly opposite pairs. This is not, in general, true of the singular case.

7. Since six planes intersect two by two in fifteen lines, every hexahedron must have, associated with it, three external lines of intersection.

(a) If these three associated lines lie in one plane, they must intersect in three points, and then the hexahedron will have six diagonal planes and four diagonal lines all intersecting in one

point. This species may be considered as formed by the planes of two tetrahedra having a common base.

(*b*) If these three lines intersect in two points, but do not lie in the same plane, there will in general be four diagonal planes only, and the four linear diagonals will intersect in four points not in the same plane.

(*c*) If only two of the three lines intersect, there will in general be only two diagonal planes, upon each of which one pair only of diagonal lines will intersect.

(*d*) Lastly, if none of the three lines meet, there will in general be no diagonal planes, and the four diagonal lines will not meet.

Each of these four species may, however, have singular cases.

8. In species (*a*) all parallel or subcontrary planes, which divide or cleave the hexahedron into two other hexahedra, have similar quadrilaterals traced upon them. In the other three species this is only true, in general, of selected planes. In the singular case of the inscribed tetrahedron, the quadrilaterals obtained by parallel cleavage are not always similar.

9. The hexahedron inscribed in the sphere belongs in general, as has been seen, to species (*a*). Its two tetrahedra have the corresponding plane angles at their vertices supplementary each to those of the other. The singular case is a very restricted singular form of species (*c*), or it may be looked upon as an indeterminate form, arising out of singularity, in species (*a*); *i. e.* that when two of the three associated lines are parallel, the third may leave their plane, under certain conditions of symmetry.

30 Scarsdale Villas, Kensington, W.,
October 18, 1861.

L. *Notices respecting New Books.*

An Elementary Treatise on the Theory of Equations, with a Collection of Examples. By I. Todhunter, *M.A.*

MR. TODHUNTER'S merits, as a writer of some of our best elementary treatises on mathematics, are now so well established as to render it quite unnecessary to dwell upon the manner in which this, his last task, has been performed. It will suffice, therefore, to inform the mathematical student that a thoroughly trustworthy, complete, and yet not too elaborate, treatise on the Theory of Equations is now within his reach; that, as far as the elementary character of the work would permit, the treatment of the subject has been brought up to the level of the science of our day; and that, in some branches of the subject, the more elaborate researches of modern authors have been carefully examined, their suitable portions judiciously selected, and now for the first time collected.

The three chapters on Determinants will be particularly accept-

able; for, except in larger treatises especially devoted to the subject, the student will nowhere find the first principles of this beautiful and powerful method so clearly and satisfactorily explained. The only suggestion that occurs to us with respect to these chapters is that they might be transferred with advantage to future editions of the author's Treatise on Algebra; for experiment has long since convinced us that the method of determinants may be introduced with great profit even in schools, and as soon as simple equations involving two or more unknown quantities are studied. We have found that pupils of average intelligence rapidly acquire a knowledge of the more elementary properties of determinants, and that they invariably regard the method as a welcome augmentation of their computing power. More important than this, however, is the fact that, as a mental discipline, the study of the properties in question is certainly not inferior to that of any other branch of algebra.

In heartily recommending the work, we will merely add that it is enriched by a collection of well-chosen examples.

LI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 324.]

November 22, 1860.—Major-General Sabine, R.A., Treasurer and Vice-President, in the Chair.

THE following communications were read:—

"Researches on the Phosphorus-Bases." No. X.—Metamorphoses of Bromide of Bromethylated Triethylphosphonium. By A. W. Hofmann, LL.D., F.R.S. Received July 24, 1860.

Among the several products of transformation into which the bromide of bromethyl-triethylphosphonium is converted when submitted to the action of reagents, the substances formed by its union with bodies similar to ammonia, have hitherto almost exclusively occupied my attention. I have, however, of late examined a variety of other changes of this body, which deserve to be noticed.

When heated, the bromide begins to evolve hydrobromic acid at a temperature of about 200°, which continues for a considerable length of time. The product of this reaction is evidently the bromide of vinyl-triethylphosphonium,

$$[(C_2H_4Br)(C_2H_5)_3P]Br = HBr + [(C_2H_3)(C_2H_5)_3P]Br.$$

It is, however, difficult to obtain the substance pure by this process, since the temperature at which the last portion of hydrobromic acid is eliminated closely approximates the degree of heat at which the vinyl-body is entirely destroyed; and since the latter compound may be obtained with the greatest facility by other processes*, I have not followed up any further this direction of the inquiry.

* The hydrated di-oxide of ethylene-hexethyl-diphosphonium, when submitted to distillation, undergoes decomposition; two different phases are to be distinguished in this metamorphosis. At about 200° the base begins to disengage the

I have already mentioned, in a previous note, the deportment of the bromethylated bromide with oxide of silver; the whole of the bromine is eliminated in the form of bromide of silver, a new base being formed.

According to circumstances, this base may be the vinyl-compound previously mentioned, or another body differing from the latter by containing the elements of one molecule of water in addition. This substance, which is always formed when the reaction takes place in moderately dilute solutions, is the oxide of a phosphonium, with three molecules of ethyl substituted for three equivalents of hydrogen, the fourth equivalent of hydrogen being replaced by an oxygenated radical C_2H_5O, arising from the radical C_2H_4Br by the insertion of HO in the place of Br

$$[(C_2H_4Br)(C_2H_5)_3P]Br+2\left.\begin{matrix}Ag\\H\end{matrix}\right\}O=2AgBr+\left.\begin{matrix}[(C_2H_4HO)(C_2H_5)_3P]\\H\end{matrix}\right\}O.$$

I have fixed the nature of this compound by the analysis of the iodide, of the platinum-salt and of the gold-salt, and, moreover, by the study of several remarkable transformations which it undergoes when submitted to the action of reagents.

It appeared of some interest to ascertain whether the *oxethylated* might be reconverted into the *bromethylated* base. The chloride of the former is energetically attacked by pentabromide of phosphorus; oxybromide of phosphorus and hydrobromic acid are abundantly evolved, and the residue of the reaction contains the chloride of bromethylated triethylphosphonium.

$$[(C_2H_5O)(C_2H_5)_3P]Cl+PBr_5=HBr+POBr_3+[(C_2H_4Br)(C_2H_5)_3P]O.$$

Thus it is seen that the molecular group C_2H_5O, which we assume as hydrogen-replacing in this salt, suffers under the influence of pentabromide of phosphorus, alterations identical with those which it is known to undergo under similar circumstances, when conceived as a constituent of alcohol.

If we consider the facility with which the bromethylated triethylphosphonium is converted into the oxethylated compound, by the action of oxide of silver, and the simple re-formation of the first-mentioned body by means of pentabromide of phosphorus, a great variety of new experiments suggest themselves. As yet but little

vapour of triethylphosphine, the residuary solution retaining hydrated oxide of vinyl-triethylphosphonium,

$$\left.\begin{matrix}[(C_2H_4)''(C_2H_5)_6P]''\\H_2\end{matrix}\right\}O_2=(C_2H_5)_3P+H_2O+\left.\begin{matrix}[(C_2H_3)(C_2H_5)_3P]\\H\end{matrix}\right\}O,$$

the latter yielding at a higher temperature the oxide of triethylphosphine together with ethylene,

$$\left.\begin{matrix}[(C_2H_3)(C_2H_5)_3P]\\H\end{matrix}\right\}O=C_2H_4+(C_2H_5)_3PO.$$

The vinyl-compound is even more readily obtained by the action of silver-salts, such as acetate of silver, at the temperature of 100°, on the bromethylated bromide.

$$[(C_2H_4Br)(C_2H_5)_3P]Br+2\left[\left.\begin{matrix}(C_2H_3O)\\Ag\end{matrix}\right\}O\right]=2AgBr+\left.\begin{matrix}[(C_2H_3)(C_2H_5)_3P]\\(C_2H_3O)\end{matrix}\right\}O+\left.\begin{matrix}(C_2H_3O)\\H\end{matrix}\right\}$$

progress has been made in this direction; one of the reactions, however, which I have studied deserves even now to be mentioned.

The salts of bromethylated and oxethylated triethylphosphonium may be regarded as tetrethyl-phosphonium-salts, in which an equivalent of hydrogen in one of the ethyl-molecules is replaced by bromine and by the molecular group HO respectively.

Bromide of tetrethylphosphonium $[(C_2H_4H)(C_2H_5)_3P]Br$,

Bromide of bromethylated triphosphonium $[(C_2H_4Br)(C_2H_5)_3P]Br$;

Bromide of oxethylated triethylphosphonium $[(C_2H_4HO)(C_2H_5)_3P]Br$;

and the question arose, whether the bromethylated compound might not be converted, simply by reduction, into a salt of tetrethylphosphonium. This transformation may, indeed, be effected without difficulty. On acidulating the solution of the bromethylated bromide with sulphuric acid and digesting the mixture with granulated zinc, the latent bromine is eliminated as hydrobromic acid, its place being at the same time filled by 1 equiv. of hydrogen,

$$[(C_2H_4Br)(C_2H_5)_3P]Br+2H=HBr+[(C_2H_5)_4P]Br.$$

It was chiefly the facility with which a tetrethyl-phosphonium-compound may be obtained from the brominated bromide, that induced me to designate the hydrogen-replacing molecules C_2H_4Br, and C_2H_5O, which we meet with in the compounds above described, as *bromethyle* and *oxethyle*. It remained to be ascertained whether these compounds might actually be formed by means of direct substitution-products of ethyle-compounds. It was with the view of deciding this question that I have examined the deportment of triethylphosphine with the monochlorinated chloride and the monobrominated bromide of ethyle.

The former of these substances has been long known, having been investigated by Regnault many years ago; the latter had not been hitherto obtained. I have prepared it by submitting bromide of ethyle to the action of dry bromine under pressure* It is a heavy aromatic liquid boiling at 110°.

The chlorinated chloride and the brominated bromide of ethyle, although essentially different in their physical properties from dichloride and dibromide of ethylene, with which they are isomeric, nevertheless resemble the ethylene-compounds in their deportment with triethylphosphine.

In both cases the final product of the reaction is a salt of hexethylated ethylene-diphosphonium. I have identified these salts with those obtained by means of dichloride and dibromide of ethylene, both by a careful examination of their physical properties, and by the analysis of the characteristic iodide and of the platinum-salt. I have not been able to trace in the first of these reactions a salt of chlorethylated triethylphosphonium; but I have established by experiment that in the reaction between triethylphosphine and brominated bromide of

* In addition to the monobrominated bromide of ethyle, $(C_4H_4Br)Br$, there is also formed in this reaction the dibrominated bromide, $(C_4H_3Br_2)Br$.

ethyle, the formation of bromethyl-triethylphosphonium invariably precedes the production of the diphosphonium-compound.

"Researches on the Phosphorus-Bases."—No. XI. Experiments in the Methyle- and in the Methylene-Series. By A. W. Hofmann, LL.D., F.R.S. Received July 24, 1860.

In former notes I have repeatedly called attention to the transformation of the bromide of bromethylated triethylphosphonium under the influence of bases. In continuing the study of these reactions, I was led to the discovery of a very large number of new compounds, the more important ones of which are briefly mentioned in this abstract.

Hybrids of Ethylene-diphosphonium.

Action of Trimethylphosphine upon Bromide of Bromethyl-triethylphosphonium.

These two bodies act upon each other with the greatest energy, and moreover exactly in the manner indicated by theory. The resulting compound was of course examined only so far as was necessary to establish the character of the reaction.

The dibromide of the hybrid diphosphonium is more soluble than the hexethylated compound formerly described, which in other respects it resembles. Oxide of silver eliminates the extremely caustic base

$$C_{11}H_{30}P_2O_2=\left.\begin{matrix}[(C_2H_4)''(CH_3)_3(C_2H_5)_3P_2]''\\ H_2\end{matrix}\right\}O_2,$$

which yields with hydrochloric acid and dichloride of platinum a pale-yellow platinum-salt,

$$C_{11}H_{28}P_2Pt_2Cl_6=\left[(C_2H_4)''\begin{matrix}(CH_3)_3P\\(C_2H_5)_3P\end{matrix}\right]''Cl_2,2PtCl_2,$$

separating in scales from boiling water.

The salts of the hybrid diphosphonium crystallize like those of the hexethylated diphosphonium, but, so far as they have been examined, are somewhat more soluble. This remark applies especially to the iodide.

It seemed worth while to try whether the bromide of bromethylated triethylphosphonium was capable of fixing a molecule of phosphoretted hydrogen. It was found, however, that the two bodies do not act upon one another. Phosphoretted hydrogen gas, passed through the alcoholic solution of the bromide, either cold or boiling, did not seem to affect it in any way.

Action of Trimethylphosphine on Dibromide of Ethylene.

This reaction exhibits a repetition of all the phenomena observed in that which takes place between the dibromide and triethylphosphine. The process is completed sooner, if possible, than in the ethyle-series. The lower boiling-point and the overpowering odour of trimethylphosphine render it advisable to mix the materials with considerable quantities of alcohol or ether; and on account of the extreme oxidability of the phosphorus-compound, it is best to ope-

rate in vessels filled with carbonic acid and subsequently sealed before the blowpipe. After digestion for a short time at 100°, the mixture of the two liquids solidifies to a hard, dazzling, white, crystalline mass containing the two bromides,

$$C_5 H_{13} P Br_2 = [(C_2 H_4 Br)(C H_3)_3 P] Br,$$

$$C_8 H_{22} P_2 Br_2 = \left[(C_2 H_4)'' \begin{matrix} (C H_3)_3 P \\ (C H_3)_3 P \end{matrix} \right]'' Br_2,$$

one or the other predominating according to the proportions in which the two bodies were allowed to act upon one another.

It was not difficult to establish the nature of these two compounds by numbers.

The solution of the saline mass in absolute alcohol, deposits, on cooling, beautiful prismatic crystals, consisting of the bromide of bromethyl-trimethylphosphonium almost chemically pure, while the diphosphonium-bromide remains in solution. The nature of the monophosphonium-compound was fixed by a bromine determination in the bromide, and by the analysis of a platinum-salt beautifully crystallized in needles containing

$$C_5 H_{13} Br P PtCl_3 = [(C_2 H_4 Br)(C H_3)_3 P] Cl, PtCl_2.$$

Treatment of this platinum-salt with sulphuretted hydrogen yielded an extremely soluble and deliquescent chloride, which was not analysed, but submitted to the action of oxide of silver, when it furnished the oxide of the corresponding oxethylated compound

$$C_5 H_{15} PO_2 = \left. \begin{matrix} [(C_2 H_5 O)(C H_3)_3 P] \\ H \end{matrix} \right\} O.$$

The caustic liquid was converted by hydrochloric acid into the easily soluble chloride corresponding to the oxide; and this chloride, when treated with dichloride of platinum, deposited the platinum-salt of the oxethylated trimethylphosphonium in well-formed octahedra extremely soluble in water, containing

$$C_5 H_{14} P O PtCl_3 = [(C_2 H_5 O)(C H_3)_3 P] Cl, PtCl_2.$$

Salts of Hexmethylated Ethylene-diphosphonium.

Dibromide.—The preparation of this salt has already been mentioned. It is extremely soluble in water, and even in absolute alcohol, insoluble in ether. *In vacuo* over sulphuric acid it solidifies into a mass of acicular crystals, which are exceedingly deliquescent.

The dibromide, treated with oxide of silver, yields the corresponding dioxide,

$$C_8 H_{24} P_2 O_2 = \left. \begin{matrix} [(C_2 H_4)'' (C H_3)_6 P_2]'' \\ H_2 \end{matrix} \right\} O_2,$$

which forms with acids a series of salts resembling the corresponding ethyle-compounds. Of these I have prepared only the

Di-iodide, which crystallizes in difficultly soluble needles of the composition

$$C_8 H_{22} P_2 I_2 = \left[(C_2 H_4)'' \begin{matrix} (C H_3)_3 P \\ (C H_3)_3 P \end{matrix} \right]'' I_2,$$

surpassing in beauty the corresponding ethyle-compound; and the

Platinum-salt.—This is an apparently amorphous precipitate, which is nearly insoluble in water, dissolves with extreme slowness in boiling hydrochloric acid, and separates therefrom on cooling in golden-yellow laminæ, very much like those of the platinum-salt of the hybrid ethylene-trimethyl-triethyl-diphosphonium. It consists of—

$$C_8 H_{22} P_2 Pt_2 Cl_6 = \left[(C_2 H_4)'' \begin{matrix} (C H_3)_3 P \\ (C H_3)_3 P \end{matrix} \right]'' Cl_2, 2PtCl_2.$$

Methylene Group.

Action of Triethylphosphine on Di-iodide of Methylene.

Triethylphosphine and di-iodide of methylene act so powerfully on one another, that it is necessary to moderate the reaction by the presence of a considerable quantity of ether. The reaction is very soon completed, even when the mixture is largely diluted, especially if it be heated to 100° in sealed tubes. The saline residue left after the evaporation of the ether is immediately seen to be a mixture of several compounds, one of which—a sparingly soluble iodide crystallizing in long needles—at once arrests attention.

From analogy we might expect to find in the saline mixture the compounds

$$[(C H_2 I)(C_2 H_5)_3 P]I,$$

or

$$[(C H_2)''(C_2 H_5)_6 P_2]'' I_2.$$

Experiment has, however, established the presence of the first only.

The difficultly soluble crystals just mentioned are easily purified, being readily soluble in water, sparingly in alcohol, insoluble in ether. Their solution in boiling alcohol yields splendid needles frequently an inch long, and possessing extraordinary lustre. Analysis prove this beautiful salt to be the first of the above-mentioned compounds.

The new iodide behaves with nitrate of silver like the bromide of bromethylated triethylphosphonium; half the iodine is eliminated in the form of iodide of silver. It differs, however, from the bromide in its deportment with oxide of silver which, after removal of the accessible iodine, leaves the latent iodine untouched, even after protracted ebullition. A powerfully alkaline solution is thus obtained containing the base

$$C_7 H_{17} I P O = \left\{ \begin{matrix} [(C H_2 I)(C_2 H_5)_3 P] \\ H \end{matrix} \right\} O.$$

The crystals of the iodide were transformed into the chloride by means of chloride of silver, and the solution was precipitated by dichloride of platinum. The precipitate is very sparingly soluble in cold water, but may be recrystallized from a considerable quantity of boiling water. As the liquid cools, splendid needle-shaped crystals are deposited containing

$$C_7 H_{17} I P Pt Cl_3 = [(C H_2 I)(C_2 H_5)_3 P]Cl, PtCl_2.$$

The sparingly soluble iodide is present in proportionally small quantity only among the products of the action of di-iodide of methylene on triethylphosphine. I have in vain endeavoured to detect among these products anything of the nature of a diphosphonium-compound. On treating the mother-liquor of the sparingly soluble iodide with chloride of silver, and the dilute filtered solution with dichloride of platinum, a few needles of the iodated platinum-salt are still deposited; but after considerable evaporation the solution yields crystals, all of which exhibit an octahedral habitus. I was equally unsuccessful in a particular experiment, in which I subjected di-iodide of methylene to the action of a large excess of triethylphosphine; and a similar report must be made of the attempt to produce the desired body by treating the ready prepared iodide with triethylphosphine, according to the equation

$$[(C H_2 I)(C_2 H_5)_3 P]I + (C_2 H_5)_3 P = [(C H_2)'' (C_2 H_5)_6 P_2]'' I_2.$$

The examination of the mother-liquor of the sparingly soluble iodide is a difficult and thankless proceeding; nevertheless, by a sufficient number of iodine- and platinum-determinations, it may be shown to be a mixture of four different compounds. The mother-liquor is thus found to contain, together with the hydriodate of the phosphorus-base, two crystallizable iodides differing in solubility, and to be separated from one another only by a great number of crystallizations.

The more soluble salt is the iodide of oxymethylated triethylphosphonium, corresponding to the iodomethylated compound; the less soluble salt is the iodide of methyl-triethylphosphonium. The last mother-liquors contain considerable quantities of oxide of triethylphosphine.

Iodide of Oxymethyl-triethylphosphonium.

This salt is extremely soluble both in water and in alcohol, even in absolute alcohol, and crystallizes only after the alcohol has been completely evaporated. The crystals, resembling the frosty efflorescences on a window-pane, contain

$$C_7 H_{18} O P I = [(C H_3 O) (C_2 H_5)_3 P] I.$$

The iodide, treated with oxide of silver, is converted into the corresponding caustic oxide, which, when mixed with hydrochloric acid and dichloride of platinum, yields a rather easily soluble platinum-salt of an octahedral habitus.

Iodide of Methyl-triethylphosphonium.

The nature of the less soluble iodide was determined by an iodine-determination, and by the analysis of the platinum-salt. The iodide dissolves in water and in alcohol, but is insoluble in ether. By adding ether to the alcoholic solution, tolerable crystals are obtained. This compound is most conveniently purified by precipitating the alcoholic mother-liquor, after freeing it by crystallization as far as possible from the iodomethylated iodide, with a quantity of ether insufficient to precipitate the whole, so that the greater part of the iodides may remain in solution.

The iodide thus prepared contains

$$C_7 H_{19} P I = [(C H_3)(C_2 H_5)_3 P]I.$$

For further verification of this formula the crystals were deiodized with silver-oxide, and the caustic liquid thus obtained was saturated with hydrochloric acid and precipitated by dichloride of platinum. The platinum-salt, which crystallizes in beautiful octahedra, was found to contain

$$C_7 H_{19} P PtCl_3 = [(C H_3)(C_2 H_5)_3 P]Cl, PtCl_2.$$

The two iodides are accompanied by a considerable quantity of oxide of triethylphosphine, which immediately separates in oily drops on treating the last mother-liquor with potash. Its presence was likewise unmistakeably recognized by the preparation of the platinum-salt. If the last mother-liquor of the iodine-compounds be deiodized and mixed with hydrochloric acid and dichloride of platinum, a quantity of octahedral salts separates in the first place, which are removed by sufficient concentration; the remaining liquid, when mixed with alcohol and ether, yields a crystalline precipitate, which separates from alcohol by spontaneous evaporation in the beautiful large hexagonal tables consisting of the platinum-salt of the oxychloride of triethylphosphine, which has been more fully described in one of the previous notes on these researches.

The formation of the four compounds contained in the mother-liquor of the sparingly soluble iodide is illustrated by the following equations :—

$$2[(C_2H_5)_3P] + CH_2I_2 + H_2O = [(C_2H_5)_3HP]I + [(CH_3O)(C_2H_5)_3P]I$$

$$[(3C_2H_5)_3P] + CH_2I_2 + H_2O = [(C_2H_5)_3HP]I + [(CH_3)(C_2H_5)_3P]I + (C_2H_5)_3P$$

"Researches on the Phosphorus-Bases."—No. XII. Relations between the Monoatomic and the Polyatomic Bases. By A. W. Hofmann, LL.D., F.R.S. Received August 17, 1860.

In recording my experiments on the derivatives of triethylphosphine, I have had more than one opportunity of alluding to the energy and precision which characterize the reactions of this compound. The usefulness of triethylphosphine as an agent of research has more particularly manifested itself in the study of the polyatomic bases, the examination of which, in continuation of former inquiries, was naturally suggested by the beautiful researches on the polyatomic alcohols published during the last few years. In the commencement these studies were almost exclusively performed with reference to derivatives of ammonia; but the results obtained in the examination of triethylphosphine have, in a great measure, changed the track originally pursued, and of late I have generally preferred to solve the problems which I had proposed to myself, by the aid of the phosphorus-bases.

The light which the study of these compounds throws upon the nature of the polyatomic bases generally, will be fully appreciated by a retrospective glance at the deportment of triethylphosphine under the influence of dibromide of ethylene, and a comparison of

the products formed in this reaction with the results suggested by theory.

A simple consideration shows that the action of diatomic bromides upon bases must give rise to the formation of several classes of compounds. Let us examine by way of illustration the products which may be expected to be formed in the reaction between ammonia and dibromide of ethylene.

The diatomic bromide being capable of fixing two molecules of ammonia, we have in the first place four diatomic bromides of the formulæ

$$[(C_2H_4)'' \; H_6N_2]'' \; Br_2$$
$$[(C_2H_4)_2'' \; H_4N_2]'' \; Br_2$$
$$[(C_2H_4)_3'' \; H_2N_2]'' \; Br_2$$
$$[(C_2H_4)_4'' \; N_2]'' \; Br_2.$$

These are, however, by no means the only salts which, in accordance with our present conception of diatomic compounds, may be formed in this reaction. Taking into consideration the general deportment of dibromide of ethylene, there could be no doubt that, under certain conditions, this body would act with ammonia as a monoatomic compound, giving rise to another series of bodies, in which the hydrogen would be more or less replaced by the monoatomic molecule C_2H_4Br, viz.

$$[(C_2H_4Br) \; H_3N]Br$$
$$[(C_2H_4Br)_2 \; H_2N]Br$$
$$[(C_2H_4Br)_3 \; HN]Br$$
$$[(C_2H_4Br)_4 \; N]Br.$$

Further, if the reaction took place in the presence of water, it was to be expected that the latent bromine of these salts, wholly or partially eliminated in the form of hydrobromic acid, would be replaced by the molecular residue of water, and thus, independently of any mixed compounds containing simultaneously bromine and oxygen, a series of salts might be looked for, in which a molecule $C_2H_4(HO) = C_2H_5O$ would enter monoatomically.

$$[(C_2H_5O) \; H_3N]Br$$
$$[(C_2H_5O)_2 \; H_2N]Br$$
$$[(C_2H_5O)_3 \; HN]Br$$
$$[(C_2H_5O)_4 \; N]Br.$$

Lastly, remembering the tendency exhibited by ethylene-compounds to resolve themselves in the presence of alkalies into vinyl-products, it appeared not improbable that a fourth series of bodies would likewise be formed,

$$[(C_2H_3) \; H_3N]Br$$
$$[(C_2H_3)_2 \; H_2N]Br$$
$$[(C_2H_3)_3 \; HN]Br$$
$$[(C_2H_3)_4 \; N]Br.$$

In the experiments on the action of dibromide of ethylene upon ammonia, which I have already partly published, and which, in a

more connected form, I hope soon to lay before the Royal Society, I have not, indeed, met with the whole of these compounds; but in the place of the deficient members of the groups new products have made their appearance, whose formation in the present state of our knowledge could scarcely have been predicted, and thus the problem of disentangling the difficulties of this reaction becomes a task of very considerable difficulty. Nor did the action of dibromide of ethylene upon ethylamine, diethylamine, and triethylamine, which I subsequently studied, afford a sufficiently simple expression of the transformations suggested by theory. The difficulties disappeared at once when the experiment was repeated in the phosphorus-series. In the reaction with dibromide of ethylene, the sharply-defined characters of triethylphosphine exhibited themselves with welcome distinctness, and in consequence more especially of the absence of unreplaced hydrogen—whereby the formation of a large number of compounds of subordinate theoretical interest was excluded—the general character of the reaction, the recognition of which was the object of the inquiry, became at once perceptible.

I have shown that the action of dibromide of ethylene upon triethylphosphine gives rise to the formation of four different compounds, viz.

$$\begin{array}{ll}
[(C_2H_4)'' & (C_2H_5)_6P_2]''Br_2 \\
[(C_2H_4Br)(C_2H_5)_3P] & Br \\
[(C_2H_5O)(C_2H_5)_3P] & Br \\
[(C_2H_3) & (C_2H_5)_3P] \; Br,
\end{array}$$

each of which represents one of the *four* groups of compounds, which under favourable circumstances may arise from the mutual reaction between ammonia and dibromide of ethylene, the production of a greater number of terms being impossible on account of the ternary substitution of triethylphosphine.

Whilst going on with the researches on the phosphorus-bases which I have taken the liberty of submitting to the Royal Society, in notes sketched as I advanced, I have not altogether lost sight of the experiments in the nitrogen-series, which had originally suggested these inquiries. Numerous nitrogenated bases, both monoatomic and diatomic, with which I have become acquainted during this investigation, must be reserved for a future communication. I may here only remark, that these substances, although differing in several points, nevertheless imitate in their general deportment so closely the corresponding terms of the phosphorus-series, that the picture which I have endeavoured to delineate of the phosphorus-compounds, illustrates in a great measure the history of the nitrogen-bodies.

In conclusion, a few words about the further development of which the experiments on the polyatomic bases appear to be capable, and about the direction in which I propose to pursue the track which they have opened.

Conceived in its simplest form, the transition from the series of

monoatomic to that of diatomic bases, may be referred to the introduction of a monochlorinated or a monobrominated alcohol-radical into the type ammonia, the chlorine and bromine thus inserted furnishing the point of attack for a second molecule of ammonia.

If in bromide of ethylammonium—to pass from the phosphorus-series to the more generally interesting nitrogen-series—we replace 1 equiv. of the hydrogen in ethyle by bromine, we arrive at bromide of bromethylammonium, which fixing a second equivalent of ammonia, is converted into the dibromide of ethylene-diammonium, the latent bromine becoming accessible to silver-salts.

$$[(C_2H_4Br)H_3N]Br + H_3N = [(C_2H_4)''H_6N_2]''Br_2.$$

The further elaboration of this reaction indicates two different methods for the construction of the polyatomic bases of a higher order. In the first place, the number of ammonia-molecules, to be incorporated in the new system, may be increased by the gradually advancing bromination of the radical. By the further bromination of ethyle in bromide of bromethylammonium and the action of ammonia on the bodies thus produced, the following salts may be generated:—

$$[(C_2H_3Br_2)H_3N]Br + 2H_3N = [(C_2H_3)'''H_9N_3]'''Br_3$$
$$[(C_2H_2Br_3)H_3N]Br + 3H_3N = [(C_2H_2)''''H_{12}N_4]''''Br_4$$
$$[(C_2HBr_4)H_3N]Br + 4H_3N = [(C_2H)'''''H_{15}N_5]'''''Br_5$$
$$[(C_2Br_5)H_3N]Br + 5H_3N = [(C_2)''''''H_{18}N_6]''''''Br_6.$$

Again, the fixation of the ammonia-molecules may be attempted, not by the progressive bromination of the ethyle, but by the accumulation of monobrominated ethyle-molecules in the ammonium-nucleus. The bromide of di-bromethylammonium, when submitted to the action of ammonia, would thus yield the tribromide of a triammonium; the bromide of tri-bromethylammonium, the tetrabromide of a tetrammonium; and lastly, the bromide of tetrabromethylammonium, the pentabromide of a pentammonium.

$$[(C_2H_4Br)_2H_2N]Br + 2H_3N = [(C_2H_4)_2''H_8N_3]'''Br_3$$
$$[(C_2H_4Br)_3HN]Br + 3H_3N = [(C_2H_4)_3''H_{10}N_4]''''Br_4$$
$$[(C_2H_4Br)_4N]Br + 4H_3N = [(C_2H_4)_4''H_{12}N_5]''''Br_5.$$

As yet the bromination of the alcohol-bases presents some difficulty; appropriately selected reactions, however, will doubtless furnish the several brominated bases. They may probably be obtained by indirect processes, similar to those by which years ago I succeeded in preparing the chlorinated and brominated derivatives of phenylamine; or these bodies may be generated by the action of pentachloride or pentabromide of phosphorus upon the oxethylated bases, a process, which, to judge from the few experiments recorded in one of the preceding sketches, promises a rich harvest of results.

I have but a faint hope that I may be able to trace these new paths in the numerous directions which open in a variety at once tempting and perplexing. Inexorable experiment follows but slowly the flight of light-winged theory. The commencement is never-

theless made, and even now the triammonium- and tetrammonium-compounds begin to unfold themselves in unexpected variety. One of the most remarkable compounds belonging to the triammonium-group is *diethylene triamine*,

$$C_4 H_{13} N_3 = \left.\begin{matrix}(C_2 H_4)''_2 \\ H_5\end{matrix}\right\} N_3.$$

This base, *the first triacid triammonia*, forms splendid salts of the formula

$$[(C_2 H_4)''_2 H_3 N_3]''' Cl_3,$$

which will be the subject of a special communication.

December 6.—Major-General Sabine, Treasurer and Vice-President, in the Chair.

The following communication was read:—

"On the Gyroscope." By Arthur Hill Curtis, Esq.

The object of this paper is to deduce on strict mechanical principles all the known properties of the gyroscope. The only assumption made is that the velocity of rotation impressed on the instrument is very great compared with that which the attached weight would produce on it if acting alone for an instant in a direction perpendicular to the axis. The theorems which the author establishes are the following:—

Theorem I.—The curves described by the extremity of the axis of the gyroscope are a system of spherical cycloids generated by the motion of a point on the spherical radius of a circle, which, constantly remaining on the same sphere, rolls without sliding on the circumference of another fixed circle situated on the same sphere. These cycloids may be either ordinary, curtate, or prolate—including the case when the system degenerates into a circle, in which case the generating point becomes the centre of the rolling circle. Their species depends on the direction of the initial velocity communicated to the axis, the direction in which the instrument is set rotating, and the position of the attached weight; when, for instance, no initial velocity is communicated to the axis, the cycloids will be ordinary *at first*, and would continue so if the gyroscope were a perfect instrument for illustrating the motion of a body round a fixed point; but the inertia of the rings on which it is mounted, and of the attached weight, as well as the resistance of the air, after a short period has elapsed, has the effect of imparting to the axis a certain velocity which modifies the curves described by it, and at last causes the motion of the axis to become for a time sensibly one of uniform progression; it then becomes oscillatory again, the amplitudes of the oscillations being smaller than before.

Theorem II.—If the outer ring be fixed in any position so as to restrict the axis of the gyroscope to a fixed plane, the motion of the axis, when a weight is attached as above, is the same whether the instrument be set rotating or not. It is proved that the angular motion of the axis is determined by an equation of the same form as that of a circular pendulum, which does not involve the angular velocity of rotation impressed on the gyroscope.

Theorem III.—If the gyroscope be set rotating rapidly, and its axis of figure be constrained, as in Theorem II., to move very freely in a plane fixed with regard to the horizon, the axis will tend to take the position of the projection on the given plane of the line drawn through the centre of gravity of the gyroscope, parallel to the axis of the earth, in such a way that the earth and the gyroscope may turn in the same direction; while, if the axis be perfectly free, it will move exactly in the same way as the axis of a telescope directed constantly towards the same fixed star, their initial positions being supposed parallel, as established experimentally by M. Léon Foucault (Comptes Rendus, September, 1852).

To prove this theorem, the angular velocity of the earth round its axis is resolved into an equal and codirectional motion of rotation round the line through the centre of gravity of the gyroscope parallel to the earth's axis, and a motion of translation, the direction of which is constantly changing, common to all parts of the earth. Of these motions the latter is communicated to the gyroscope by the friction of its base, and does not modify its position with regard to the horizon. The first alone requires to be considered. In order to estimate its effect, a rotation equal to it and round the same axis, but in an opposite direction, must be supposed to be communicated both to the earth and the gyroscope. This does not affect their relative motion, and simplifies the problem, as it enables us to consider the earth at rest. The relative motion of the gyroscope may therefore be found by adding to the three components, round its principal axis, of its instantaneous angular velocity of rotation, as found from its equations of absolute motion, the components of this introduced velocity of rotation, the moment of resistance of the given plane being taken into account in forming the equations of motion, and its intensity supposed such as to counteract that part of the total angular velocity of the axis which is perpendicular to the given plane. The equation which determines the motion of the axis is shown to be identical with that of a circular pendulum, and the motion consequently one of oscillation, the mean position of the axis being that in which it approaches, as close as the conditions of the question permit, to the line drawn through its centre of gravity parallel to the earth's axis, and in which it rotates in a direction similar to that of the earth's rotation. Similar reasoning establishes the second part of the theorem, which is theoretically true whether the gyroscope be set rotating or not. This result is, however, in practice modified by the effects of friction; but when a rapid rotatory motion has been impressed on the gyroscope, it acquires a stability which enables it to overcome to a great extent these effects.

December 13.—Major-General Sabine, Treasurer and Vice-President, in the Chair.

The following communication was read:—

"On the Surface-condensation of Steam." By J. P. Joule, LL.D., F.R.S.

In the author's experiments steam was passed into a tube, to the outside of which a stream of water was applied, by passing it along

the concentric space between the steam-tube and a wider tube in which the steam-tube was placed. The steam-tube was connected at its lower end with a receiver to hold the condensed water. A mercury gauge indicated the pressure within the apparatus. The principal object of the author was to ascertain the conductivity of the tube under varied circumstances, by applying the formula suggested by Professor Thomson,

$$C=\frac{w}{a}\log\frac{V}{v},$$

where a is the area of the tube in square feet, w the quantity of water in pounds transmitted per hour, V and v the differences of temperature between the inside of the steam-tube, and the refrigerating water at its entrance and at its exit. The following are some of the author's most important conclusions.

1. The pressure in the vacuous space is sensibly the same in all parts.

2. It is a matter of indifference in which direction the refrigerating water flows in reference to the direction of the steam and condensed water.

3. The temperature of the vacuous space is sensibly equal in all its parts.

4. The resistance to conductivity must be attributed almost entirely to the film of water in immediate contact with the inside and outside surfaces of the tube, and is little influenced by the kind of metal of which the tube is composed, or by its thickness up to the limits of that of ordinary tubes.

5. The conductivity increases up to a limit as the rapidity of the stream of water is augmented.

6. By the use of a spiral of wire to give a rotary motion of the water in the concentric space, the conductivity is increased for the same head of water.

The author, in conclusion, gives an account of experiments with atmospheric air as the refrigerating agent; the conductivity is very small in this case, and will probably prevent air being employed for the condensation of steam except in very peculiar circumstances.

December 20.—Major-General Sabine, Treas. and V.P., in the Chair.

The following communication was read:—

"Preliminary Notice of Researches into the Chemical Constitution of Narcotine and of its Products of Decomposition." By A. Matthiessen, Esq., and George C. Foster, Esq.

I. *Composition of Narcotine.*

The announcement made by Wertheim* and Hinterberger† of the probable existence of various kinds of narcotine, rendered it necessary to commence the present investigation by a series of analyses of our material, in order to ascertain which variety of narcotine we were dealing with.

The narcotine employed was obtained from Mr. Morson, to whom

* Chem. Gaz., 1850, p. 141. † Ibid., 1851, p. 309.

we are greatly indebted for the scrupulous care bestowed on its preparation and purification. He stated that it was extracted from the residues which had accumulated during the preparation of very large quantities of morphine and codeine, from opium of various qualities and from various sources. If, therefore distinct varieties of narcotine exist, there was reason to expect that our narcotine would prove to be a mixture of several of them. The results of all our analyses, however, agree with the formula $C^{22}H^{23}NO^7$, as shown by the following Table, which gives the highest, lowest, and mean results obtained:—

	Calculated.		Found.		
			Maxima.	Minima.	Mean.
C^{22}	264	63·92	64·00	63·42	63·79
H^{23}	23	5·57	6·05	5·69	5·81
N	14	3·39	3·40	3·26	3·32
O^7	112	27·12	27·53	26·72	27·08
$C^{22}H^{23}NO^7$	413	100·00			

The formula which has been generally admitted since the publication of Wöhler's* and Blyth's† researches on narcotine, namely, $C^{23}H^{25}NO^7$, requires the following per-centages:—

Carbon	64·61
Hydrogen	5·85
Nitrogen	3·30
Oxygen	26·24

We may here remark that the recorded analyses of narcotine and its salts, with the exception of one by Dr. Hofmann, published by Blyth, agree at least as well with the former as with the latter formula; moreover, during the course of experiments made with several pounds of narcotine, we have observed nothing, either in the behaviour of this base itself, or in the nature or proportions of its products of decomposition, to indicate that it was variable in composition. Further data are, however, needed for the final decision of this question, and we shall accordingly feel very much indebted to any chemist who has a specimen of narcotine of well-ascertained origin, or which he believes to have a different composition from that given above, if he will kindly spare us a sufficient quantity for analysis.

II. *Composition of Cotarnine.*

The combustion of cotarnine with oxide of copper and oxygen, as well as the determination of the proportion of platinum in its chloroplatinate, leads us to adopt the formula $C^{12}H^{13}NO^3$ for this base. The formula usually adopted contains one more atom of carbon; but, independently of our analytical results, the supposition that cotarnine contains only twelve atoms of carbon is supported by the simple manner in which the action of oxidizing substances on narcotine can then be expressed, namely, by the equation

$$\underset{\text{Narcotine.}}{C^{22}H^{23}NO^7} + O = \underset{\text{Opianic acid.}}{C^{10}H^{10}O^5} + \underset{\text{Cotarnine.}}{C^{12}H^{13}NO^3},$$

* Ann. Chem. Pharm. vol. l. p. 1.

† Phil Mag. S. 3. vol. xxv. p. 363.

and, as will be shown hereafter, by the manner in which cotarnine is decomposed by dilute nitric acid.

III. *Decompositions of Opianic Acid.*

Opianic acid is readily decomposed when heated with strong hydriodic acid; no iodine is set free, but iodide of methyle is formed in considerable quantity at the same time as a non-volatile substance, very easily altered by heat and exposure to air, especially if in contact with alkali, the precise nature of which we have not yet been able to ascertain.

When opianic acid is heated with an excess of a very strong solution of potash, it splits up into meconine and hemipinic acid. These substances were found by experiment to be formed in proportions corresponding to the equation

$$\underset{\text{Opianic acid.}}{2C^{10}H^{12}O^{5}} = \underset{\text{Meconine.}}{C^{10}H^{10}O^{4}} + \underset{\text{Hemipinic acid.}}{C^{10}H^{10}O^{6}}.$$

The meconine thus produced has all the characters which have been ascribed by previous observers to meconine obtained by other processes; its identity was further established by analysis, and by the preparation of chloro- and nitro-meconine, the former of which was analysed. The hemipinic acid was also found to be identical with that obtained irectly from narcotine: the acid and its silver-salt were analysed.

Having thus found a method by which meconine and hemipinic acid can be produced with certainty and in large quantities, we intend to make an extended investigation of them and of opianic acid, in the hope of discovering the nature of the relationship of these three bodies to each other and to narcotine. The principal results which we have hitherto obtained in this direction are as follows.

Action of Hydriodic Acid on Meconine.—Meconine is decomposed by hydriodic acid like opianic acid, giving iodide of methyle and an easily alterable substance, the nature of which has not been determined.

Action of Hydriodic Acid on Hemipinic Acid.—Hemipinic acid, heated with concentrated hydriodic acid to within a few degrees of the boiling-point of the latter substance, is decomposed into iodide of methyle, carbonic acid, and an acid of the formula $C^{7}H^{6}O^{4}$. It was found by direct experiment that two atoms of iodide of methyle are formed from each atom of hemipinic acid, so that the following equation probably represents the reaction:—

$$\underset{\text{Hemipinic acid.}}{C^{10}H^{10}O^{6}} + 2HI = 2CH^{3}I + CO^{2} + \underset{\text{New acid.}}{C^{7}H^{6}O^{4}}$$

The new acid is moderately soluble in cold water, and very soluble in boiling water, alcohol, and ether; its solution has a strongly acid reaction with test-paper. It separates from hot water in small needle-shaped crystals containing 14·80 per cent. water of crystallization, which they lose at 100° (the formula $C^{7}H^{6}O^{4} + 1\frac{1}{2}H^{2}O$ corresponds to 14·92 per cent. water); at a higher temperature the acid melts and sublimes without apparent alteration.

Dried at 100°, it gave the following results on analysis:—

	Calculated.		Found. (mean)
C^7	84	54·55	54·38
H^6	6	3·89	3·91
O^4	64	41·56	41·71
	154	100·00	100·00

When the dry acid is heated in the air to a little above 100°, it slowly oxidizes and becomes brown; the same change takes place more rapidly when a solution of it, especially if neutral or alkaline, is evaporated. A solution of the acid immediately reduces ammonio-nitrate of silver, even in the cold; with sulphate of copper and a slight excess of potash it gives a yellowish-green solution, from which suboxide of copper is precipitated on warming. The free acid, or its ammonia-salt, gives a very intense blue coloration with perchloride of iron. The colour thus produced is changed to blood-red (exactly resembling the red produced by the sulphocyanates) by ammonia, and is destroyed by strong acids, being restored by dilution with water, or by neutralization by an alkali: like the colouring matter obtained by Anderson by the action of sulphuric acid on opianic acid, it is entirely removed from solution by alumina.

We have not yet obtained any of the salts of the new acid in a state fit for analysis, and prefer not to propose a name for it until its relationship to other bodies has been more thoroughly examined; its formula, however, assigns to it a place in the following series—

$C^7 H^6 O$	Oil of bitter almonds.
$C^7 H^6 O^2$	Benzoic acid.
$C^7 H^6 O^3$	Salicylic acid.
$C^7 H^6 O^4$	New acid.
$C^7 H^6 O^5$	Gallic acid.
$C^7 H^6 O^6$	Tannoxylic acid (?).

It is remarkable that salicylic and gallic acids both give colorations with perchloride of iron much resembling that produced by the acid $C^7 H^6 O^4$.

IV. *Action of dilute Nitric Acid on Cotarnine.*

By gently heating cotarnine with very dilute nitric acid, we have obtained nitrate of methylamine and a new acid, *cotarnic acid*, but have not hitherto found out the conditions necessary for the certain production of the latter substance.

Cotarnic acid dissolves easily in water, giving a solution which reacts strongly acid with litmus-paper; it dissolves only sparingly in alcohol, and is precipitated from its alcoholic solution by ether. Heated with an excess of sodium, it gives no trace of cyanide, and therefore contains no nitrogen. With perchloride of iron it gives no coloration; with acetate of lead it gives a white precipitate insoluble in excess of acetate; with nitrate of silver it gives a precipitate which is very slightly soluble in hot water. The silver-salt, crystallized from water, was found to contain $C^{11} H^{10} Ag^2 O^5$: on analysis it gave the following results:—

	Calculated.		Found (mean).
C^{11}	132	30·14	29·67
H^{10}	10	2·27	2·17
Ag^2	216	49·32	49·24
O^5	80	18·27	18·92
$C^{11}H^{10}Ag^2O^5$	438	100·00	100·00

The formation of cotarnic acid is therefore represented by the equation

$$\underset{\text{Cotarnine.}}{C^{12}H^{13}NO^3} + 2H^2O + NHO^3 = \underset{\text{Cotarnic acid.}}{C^{11}H^{12}O^5} + \underset{\text{Nitrate of methylamine.}}{N(NCH^6)O^3}$$

It is possible that the substance obtained by Anderson by the action of nitric acid on narcotine (Chem. Soc. Quart. Journ. vol. v. p. 265; Gerhardt, 'Traité,' vol. iv. p. 80), and supposed by him to be hydrate of meconine (*Opianyle*, Anderson), may have been cotarnic acid, with the composition of which Anderson's analyses closely agree, as shown by the following comparison:—

	Calculated.		Anderson.	
C^{11}	132	58·93	58·83	58·84
H^{12}	12	5·36	5·17	5·42
O^5	80	35·71	36·00	35·74
	224	100·00	100·00	100·00

If cotarnic acid be represented by the formula

$$\left.\begin{matrix}(C^{11}H^{10}O^3)''\\ H^2\end{matrix}\right\}O^2,$$

cotarnine becomes methyl-cotarnimide—

$$\left.\begin{matrix}(C^{11}H^{10}O^3)''\\ C\quad H^3\end{matrix}\right\}N;$$

if, however, we retain the formula $C^{12}H^{13}NO^3$ for cotarnine, no simple relation is apparent between it and cotarnic acid.

V. *Conclusion.*

In the absence of more definite knowledge of the constitution of meconine and opianic and hemipinic acids, it is obviously useless to try to assign a rational formula to narcotine. According to the formulæ which we have adopted for narcotine and cotarnine, narcotine contains the elements of cotarnine and meconine:—

$$\underset{\text{Narcotine.}}{C^{22}H^{23}NO^7} = \underset{\text{Cotarnine.}}{C^{12}H^{13}NO^3} + \underset{\text{Meconine.}}{C^{10}H^{10}O^4}.$$

It will be seen that these formulæ are the same as those of the methyl-narcotine and methyl-cotarnine of Hinterberger and Wertheim. The ground upon which Wertheim admitted the existence of ethyl- and propyl-narcotine was the formation of volatile bases containing C^2H^7N and C^3H^9N by the distillation of narcotine with potash. An experiment which we have made goes some way towards explaining the formation of these bases without assuming the existence of more than one variety of narcotine. Having so frequently observed the formation of methyle-compounds from the derivatives

of narcotine, we tried the direct action of hydriodic acid on this base, expecting to obtain iodide of methyle. By distilling 20 grms. of narcotine with concentrated hydriodic acid, 19 grms. of pure iodide of methyle were obtained, a quantity which corresponds, as nearly as could be expected, with three atoms of iodide of methyle for one atom of narcotine*,

$$(C^{22}H^{23}NO^{7} : 3CH^{3}I :: 413 : 436 \text{ or } 20 : 21{\cdot}1).$$

Narcotine therefore contains three atoms of methyle so combined as to be easily separable†; and it is very probable that when it is distilled with potash, according to the conditions of the experiment, sometimes nearly pure ammonia is evolved, while, at other times, methylamine, $CH^{5}N$, dimethylamine, $C^{2}H^{7}N$, or trimethylamine, $C^{3}H^{9}N$, predominates.

We wish not to close without acknowledging our obligation to Dr. M. Holzmann for very valuable assistance rendered to us at the commencement of our investigation.

GEOLOGICAL SOCIETY.

[Continued from p. 326.]

June 19, 1861.—Leonard Horner, Esq., President, in the Chair.

The following communications were read:—

1. "On the Lines of Deepest Water around the British Isles." By the Rev. R. Everest, F.G.S.

By drawing on a chart a line traversing the deepest soundings along the English Channel and the Eastern Coast of England and Scotland, continuing it along the 100-fathom-line on the Atlantic side of Scotland and Ireland, and connecting with it the line of deepest soundings along St. George's Channel, an unequal-sided hexagonal figure is described around the British Isles, and a pentagonal figure around Ireland. A hexagonal polygon may be similarly defined around the Isle of Arran. These lines were described in detail by the author, who pointed out that they limited areas similar to the polygonal form that stony or earthy bodies take in shrinking, either in the process of cooling or in drying. The relations of the 100-fathom-line to the promontories, the inlets, and general contour of the coast were dwelt upon; and the bearings that certain lines drawn across the British Isles from the projecting angles of the polygon appear to have on the strike and other conditions of the strata were described. After some remarks on the probable effect that shrinkage of the earth's crust must have on the ejection of molten rock, the author observed that, in his opinion, the action of shrinking is the only one we know of that will afford any solution of the phenomena treated of in this paper, namely, long lines of depression accompanied by long lines of elevation, often, as in the case of the British Isles, Spain and Portugal, and elsewhere, belong-

* It is possible that narcotine will prove to be an economical, as it is certainly the most convenient, source of iodide of methyle.

† Gerhardt (Traité, iv. 64) had previously observed the production of a volatile substance, which he supposed to be nitrate of ethyle or of methyle, by the action of nitric acid on narcotine.

ing to parts of huge polygons broken up into small ones, as if the surface of the earth had once formed part of a basaltic causeway.

Several charts, plans, and drawings were provided by the author in illustration of the paper.

2. "On the Ludlow Bone-bed and its Crustacean Remains." By J. Harley, M.B.

Of the two bone-beds occurring near Ludlow, the lower one (seen in Ludford Lane and on the north-east slopes of Whitcliff) is that which has supplied the author with the materials for this paper. Besides spines, teeth, and shagreen-like remains of fish, the author finds in the Ludlow Bone-bed three kinds of minute organisms: 1st, conical bodies, the same as the "Conodonts" of Pander; 2ndly, bodies somewhat like the crown of a molar tooth; 3rdly, oblong plates. All these bodies possess the same chemical composition and microscopical structure—which is decidedly *Crustacean*. With *Pterygotus* they do not appear to have any relationship, unless some are the stomach-teeth: nor do they show any alliance with Trilobites; but with *Ceratiocaris* they have a great resemblance as to structural characters, and some of them were probably the minute secondary spines of the tail of that Phyllopod. The plate-like forms might have belonged to Squilloid or Limuloid Crustaceans. To facilitate the recognition of these bodies, Mr. Harley places them all in one provisional genus with the name of *Astacoderma*. A letter from Dr. Volborth to the author was also read in confirmation of Mr. Harley's opinion that these bodies are identical with Dr. Pander's "Conodonts." Numerous original drawings illustrated the paper.

3. "On the Old Red Sandstone of Forfarshire." By James Powrie, Esq., F.G.S.

The author described the series of stratified rocks belonging to the Old Red Sandstone, upwards of 3000 feet in thickness, stretching southward from the Grampians to the coast of Fifeshire. 1st. Dark-red grits (with cornstones and flagstones) equivalent to the English "Tilestones." 2ndly. Thick conglomerates and the Arbroath paving-flags: *Pterygotus anglicus*, *Stylonurus*, *Parka decipiens*, *Cephalaspis*, *Diplacanthus gracilis*, and other fossils belong to this part of the series. 3rdly. Thick-bedded red sandstone (with cornstone): *Cephalaspis* and *Pteraspis*. 4thly. Soft deep-red sandstones. 5thly. Spotted marls and shales: these are the uppermost, and may be the equivalent of the Holoptychian beds of Clashbinnie. The author showed that between the Grampians and the trappean hills of Bunnichen and Bunbarrow the series forms a great syncline; and between these hills and the sea the older beds are twice again brought to the surface; and he believes that the marls and sandstones at Whiteness are not unconformable, as Sir C. Lyell has represented them in his published section.

4. The Secretary gave a brief account of the discovery of an exposure of sandstone strata with two bands of clay full of calcareous nodules containing plentiful remains of *Coccosteus*, *Glyptolepis*, and other fishes belonging to the Old Red Sandstone, in a burn about 2½ miles from the Manse at Edderton, Ross-shire, on the south side

of Durnoch Firth. This information was contained in a letter from the Rev. J. M. Joass, of Edderton, communicated by Sir R. I. Murchison, V.P.G.S.

5. "On the Outburst of a Volcano near Edd, on the African coast of the Red Sea." By Capt. L. R. Playfair, R.N.

At Edd, lat. 13° 57′ N., long. 41° 4′ E., about half-way between Massouah and the Straits of Bab-el-Mandel, earthquake-shocks occurred on the night of the 7th of May or the morning of the 8th, during about an hour. At sunrise fine dust fell, at first white, afterwards red; the day was pitch-dark; and the dust was nearly knee-deep. On the 9th the fall of dust abated; and at night fire and smoke were seen issuing from Jebel Dubbeh, a mountain about a day's journey inland; and sounds like the firing of cannon were heard. At Perim these sounds were heard at about 2 A.M. on the 8th, and at long intervals up to the 10th or 11th. The dust was also met with at sea; and along the entire coast of Yemen the dust fell for several days. Several shocks were felt on the 8th at Mokha and Hodaida.

6. "Notice of the occurrence of an Earthquake on the 20th of March, 1861, in Mendoza, Argentine Confederation, South America." By C. Murray, Esq.

At about ¼ to 9 o'clock, the first shock, preceded by a thunder-clap, destroyed the city of Mendoza, killing (it is said) two-thirds of its 16,000 inhabitants. Altogether there were eighty-five shocks in ten days. The land-wave appears to have come from the south-east. Several towns S.E. of Buenos Ayres felt slight shocks. No earthquake took place in Chile; but travellers crossing the Upsallata Pass of the Cordilleras met with a shower of ashes; the pass was obstructed by broken rocks; and chasms opened on all sides. At Buenos Ayres, 323 leagues from Mendoza, and elsewhere, it was observed in watch-makers' shops that the pendulums moving N. and S. were accelerated; those moving E. and W. were not affected.

7. "On the Increase of Land on the Coromandel Coast." By J. W. Dykes, Esq. In a Letter to Sir C. Lyell, F.G.S.

In the districts of the Kistna and Godavery, the land presents a parallel series of ridges and hollows near the coast, not in relation to the rivers but to the coast-line. These may have been formed by sedimentary deposits similar to what are now taking place on the Coromandel coast. By the strong currents alternately running N. and S., according to the monsoons, lines of sediment parallel with the coast are formed; and by the occasional interference of winds and tides dams are thrown across the hollows, and the latter soon become filled up. These parallel bands of coast-land become, in time, upheaved, and more or less affected by atmospheric agencies.

LII. *Intelligence and Miscellaneous Articles.*

ON A NEWLY DISCOVERED ACTION OF LIGHT.

BY M. NIÈPCE DE ST. VICTOR.

WHEN the freshly broken part of an opake porcelain plate was exposed to a strong sun for two or three hours, and then placed on choride of silver paper, after twenty-four hours' contact the silver

was found to be reduced in the part corresponding to that which had been exposed to light, but there was no reduction in that part which had been preserved from light. Certain fine specimens of porcelain acquire this activity more easily.

A steel plate polished at one part, and roughened at another by the action of aquafortis, and well cleaned by alcohol, was exposed to the sun for two or three hours under the following conditions—half the polished and unpolished plate under an opake screen, and the other half under a white glass. The plate was then covered by a paper prepared with albuminized chloride of silver. After twenty-four hours' contact, an impression was formed on the unpolished part which had been exposed to the light, but none on the polished part, nor on the unpolished part under the screen. A roughened glass plate carefully cleaned gave similar results.

These experiments show that it is not necessary for the reduction of silver salts that there be a chemical action, as when a metallic salt is insolated with an organic matter. M. Arnaudon has repeated some of these experiments with different gases, and has obtained the same results as with air.

I may here recall a previous observation, that the insolated earth exhibits traces of this action to a depth of a metre, the thickness varying, of course, with the nature of the soil and the degree of insolation. The following experiment supports this view :—In a tin tube lined with pasteboard impregnated with tartaric acid, and insolated so as strongly to reduce silver salts, I placed in the middle of the tube, but not in contact, a small bladder containing a weak solution of starch ; after forty-eight hours this starch feebly reduced Barreswil's liquor, while other starch placed in the same conditions, excepting the insolation, produced no effect on the liquor.

The following experiments were made with a view of trying whether light could magnetize a steel bar, as has frequently been stated. Avoiding all sources of error, a knitting-needle suspended by a hair was entirely unattracted by another needle insolated for a very long time in a beam of light concentrated by a strong lens, whether the light was white or had traversed a violet glass.

I then enclosed a needle in a paper impregnated with nitrate of uranium, or tartaric acid, and insolated ; I also suspended a needle horizontally in tubes containing insolated pasteboard ; and the results were always negative, as also was the case with experiments made with very feebly magnetized needles in the hope of demagnetizing them.

In conclusion, this persistent *activity* imparted by light to porous bodies cannot be the same as phosphorescence ; for, from Becquerel's experiments, it would not continue so long : it is probable that, as Foucault believes, it is a radiation invisible to our eyes, and which does not traverse glass.—*Comptes Rendus*, July 1, 1861.

ON TERRESTRIAL REFRACTION. BY M. BABINET.

A ray of light which traverses the layers of the atmosphere horizontally, is deflected from its rectilinear path towards the earth

by a quantity which, in the mean, is a fifteenth of the terrestrial arc extending from the point where the ray enters to the point at which it arrives. Thus for a horizontal path of 1852 metres, which is equal to a minute of an arc on the terrestrial globe, the deflection or refraction of the ray would be $\frac{1}{15}$ of a minute, or 4″.

There are three things to be considered in this question;—

1. The trajectory of the ray is a circle.

2. There is a constant ratio n between the quantity by which the ray is inflected, and the terrestrial arc comprised between the point of entrance of the ray taken to be horizontal, and its point of arrival. Then let s be the angle at the earth's centre comprised between these two points, and r the refraction, we have

$$\frac{r}{s}=n=\mathrm{R}(m-1)\,\frac{\mathrm{B}}{0^{m}\cdot 76}\,\frac{1}{(1+\alpha t)^{2}}\left\{\frac{1}{0^{m}\cdot 76\,d}-\frac{\alpha}{\mathrm{M}}\right\}.$$

Here R is the mean radius of the earth; B is its atmospheric pressure reduced to zero; α is the coefficient of expansion $\frac{1}{3000}$ of the air for 1° C; d is the density of mercury as compared with air taken at zero; and (what is new and important) M is the height in metres corresponding to a diminution of one degree on the Centigrade scale.

This formula, expressed numerically, becomes

$$n=\frac{\mathrm{B}}{0^{m}\cdot 76}\,\frac{1}{(1+\alpha t)^{2}}\left\{0\cdot 2345-\frac{6^{m}\cdot 867}{\mathrm{M}}\right\}.$$

Several remarkable conclusions may be deduced from this, relating to the physical constitution of the atmosphere.

3. If the ray does not travel horizontally, but is inclined to the horizon at an angle i, the atmospheric refraction diminishes in the ratio of $\cos i$ to unity; but then the path of the ray being greater than its horizontal projection in the ratio of unity to $\cos i$, a compensation is established; and calling s the angle at the centre of the earth comprised between the signal and the observer, we have, as before,

$$\frac{r}{s}=n=\frac{\mathrm{B}}{0^{m}\cdot 76}\,\frac{1}{(1+\alpha t)^{2}}\left\{0\cdot 2345-\frac{6^{m}\ 867}{\mathrm{M}}\right\}.$$

There would be no refraction, and the ray would travel in a right line, if $0\cdot 2345-\frac{6^{m}\cdot 867}{\mathrm{M}}=0$, which gives $\mathrm{M}=29^{m}\cdot 3$. Thus if the temperature of the air sank 1° for $29^{m}\cdot 3$, there would be no refraction. On the other hand, taking $\mathrm{B}=0^{m}\cdot 76$ and $t=0$, we have

$$n=0\cdot 2345-\frac{6^{m}\cdot 867}{\mathrm{M}};$$

taking this quantity as equal in the mean to $\frac{1}{15}$, or 0·0667, we have about 41 metres for M: all these quantities are much less than 200 metres, which is the height necessary to be traversed to have a diminution of one degree of temperature in the higher layers of the atmosphere.

The coefficient n varies from 0·500 to 0·000: it can even become negative, which corresponds to the case of mirage whenever M is less than $29^{m}\cdot 3$. We shall see afterwards the great influence which

the number M exerts on the stability of the atmosphere; but the formula which gives the value of n shows that in the vicinity of the soil the temperature decreases far more rapidly than aërostatic ascents would seem to indicate.

In a second note, M. Babinet gives a complete development of the above formula; and in a third note he gives a complete formula for refraction.

He supposed the heights h taken above the horizon of the observer, and not from a point of the surface corresponding to dh. Taking, as is necessary, the height h of a point of the trajectory of the ray on the vertical passing by this point, it follows that for horizontal refraction the atmospheric path is very limited, and that therefore the expression for refraction could never be a formula which becomes infinite for $z=90°$. Supposing always a decrease of 1° C. for M metres, the complete differential formula is

$$dr=dh\frac{\mathrm{R}\sin z}{\sqrt{h^2+2\mathrm{R}h+\mathrm{R}^2\cos^2 z}}\left(1+\alpha t-\frac{\alpha h}{\mathrm{M}}\right)^{\frac{\mathrm{M}}{0{\cdot}76\mathrm{D}\alpha}-2}(m-1)\frac{\mathrm{B}}{0{\cdot}76}\left(\frac{1}{0{\cdot}76\mathrm{D}}-\frac{\alpha}{\mathrm{M}}\right),$$

the integral being taken from $h=0$ to $h=\frac{\mathrm{M}(1+\alpha t)}{\alpha}$. It will probably be necessary to suppose that M varies with the height h, and to replace M by $\mathrm{M}+kh$, k being so determined that, for instance, with a height of 7000 metres, 200 metres correspond to an increase of 1°. Thus $\mathrm{M}+7000k=220$, and $k=\frac{220-\mathrm{M}}{7000}$.—*Comptes Rendus*, September 2 and 9, and October 7, 1861.

ON THE MAXIMUM DENSITY OF SEA-WATER.
BY M. V. NEUMANN.

Von Neumann, in an inaugural dissertation (Munich, 1861), has published a new determination of the maximum density of sea-water. Like Kopp and other physicists, who have made this determination for pure water, he measured the volume at different temperatures in a glass vessel analogous in construction to a thermometer, the coefficient of expansion of the glass being carefully determined. This method is well adapted for liquids whose freezing-point is above the point of greatest density. The sea-water used was obtained from Trieste, Genoa, and Heligoland, and was previously well mixed. Its freezing-point was found to be −2°·6 C., and its specific gravity at 0° C. 1·0281; its point of greatest density was −4°·7364 C.

This number is more than that obtained by Despretz (−3°·67 C. for sea-water of 1·0273 sp. gr.) and Erman (−3°·75 C.), but would probably agree with that of Marcet (−5°·25 C.) and Horner (−5°·56 C.), if a correction for the expansion of glass were introduced.—Poggendorff's *Annalen*, August 1861.

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LIII. *Explanation of a Projection by Balance of Errors for Maps applying to a very large extent of the Earth's Surface; and Comparison of this projection with other projections.* By G. B. AIRY, *Esq., Astronomer Royal**.

1. IN the projection for maps, whose principles I am about to explain, any point of the earth's surface (as Greenwich, Paris, &c.) may be adopted as the Centre of Reference, to be represented by the Central Point of the Map. But the projection which I propose, and those with which I shall compare it, are all subjected to the following conditions: that the azimuth of any other point on the earth, as viewed from the Centre of Reference, shall be the same as the azimuth of the corresponding point of the map as viewed from the central point of the map; and that equal great-circle distances of other points on the earth from the Centre of Reference, in all directions, shall be represented by equal radial distances from the central point of the map. These conditions include the Stereographic Projection, Sir H. James's Projection, and others; but they exclude Mercator's Projection, and the projections proposed by Sir John Herschel.

2. In projections like these, in which the relation of the surface represented to the surface representing it is the same in all directions from the central point, it is unnecessary for us to embarrass ourselves with considerations of the place of the pole and the forms of the curves representing arcs of meridian and parallels. It will be sufficient to consider what will be the radii of circles on the map which shall represent circles on the earth, of different radii (as measured by arcs of great circle), but having the centre of reference as their common centre; and when values are found for the radii of these circles on the map, means will

* Communicated by the Author.

easily be found for exhibiting, with their assistance, the points of intersection of meridians and parallels which are referred to the pole of the earth.

3. The two errors, to one or both of which all projections are liable, are, Change of Area, and Distortion, as applying to small portions of the earth's surface. On the one hand, a projection may be invented (to which I shall give the name of "Projection with Unchanged Areas") in which there is no Change of Area, but excessive Distortion, for parts far from the centre; on the other hand, the Stereographic Projection has no Distortion, but has great Change of Area for distant parts. Between these lie the projections which have usually been adopted by geographers, with the tacit purpose of greatly reducing the error of one kind by the admission of a small error of the other kind, but without any distinctly-expressed principle (so far as I know) for their guidance in the details of the projection.

4. My object in this paper is to exhibit a distinct mathematical process for determining the magnitudes of these errors, so that the result of their combination shall be most advantageous. This principle I call "The Balance of Errors." It is founded upon the following assumptions and inferences:—

First. The Change of Area being represented by

$$\frac{\text{projected area}}{\text{original area}} - 1,$$

and the Distortion being represented by

$$\frac{\text{ratio of projected sides}}{\text{ratio of original sides}} - 1 = \frac{\text{projected length} \times \text{original breadth}}{\text{projected breadth} \times \text{original length}} - 1$$

(where the length of the rectangle is in the direction of the great circle connecting the rectangle's centre with the Centre of Reference, and the breadth is transverse to that great circle), these two errors, when of equal magnitude, may be considered as equal evils.

Second. As the annoyance produced by a negative value of either of these formulæ is as great as that produced by a positive value, we must use some even power of the formulæ to represent the real amount of the evil of each. I shall take the squares.

Third. The total evil in the projection of any small part may properly be represented by the sum of these squares.

Fourth. The total evil on the entire Map may therefore be properly represented by the summation through the whole Map (respect being had to the magnitude of every small area) of the sum of these squares for every small area.

Fifth. The process for determining the most advantageous projection will therefore consist in determining the laws ex-

pressing the "radii of map-circles" in article 2 in terms of the "great-circle radii on the earth," which will make the total evil, represented as has just been stated, the smallest possible.

5. Now let l and b be the length and breadth of a small rectangle on the earth's surface, and suppose that the length and breadth of the corresponding rectangle on the map are $l+\delta l$ and $b+\delta b$, and neglect powers of δl and δb above the first. (Although this does not apply with algebraic correctness to very great change of area and distortion, yet it will be found by the result that the theoretical failure introduces no practical inconvenience.) Then the Change of Area

$$= \frac{\text{projected area}}{\text{original area}} - 1 = \frac{(l+\delta l).(b+\delta b)}{lb} - 1 = \frac{\delta l}{l} + \frac{\delta b}{b}.$$

And the Distortion

$$= \frac{\text{projected length}}{\text{projected breadth}} \times \frac{\text{original breadth}}{\text{original length}} - 1 = \frac{l+\delta l}{b+\delta b} \times \frac{b}{l} - 1$$
$$= \frac{\delta l}{l} - \frac{\delta b}{b}.$$

The sum of their squares, or $\left(\frac{\delta l}{l} + \frac{\delta b}{b}\right)^2 + \left(\frac{\delta l}{l} - \frac{\delta b}{b}\right)^2$, is

$$2\left(\frac{\delta l}{l}\right)^2 + 2\left(\frac{\delta b}{b}\right)^2.$$

And therefore we may use $\left(\frac{\delta l}{l}\right)^2 + \left(\frac{\delta b}{b}\right)^2$ as the measure of the evil for each small rectangle.

6. Let θ be the length, expressed in terms of radius, of the arc of great circle on the earth connecting the centre of the small rectangle with the Centre of Reference; r the corresponding distance on the map, expressed in terms of the same radius, of the projection of the centre of the small rectangle from the centre of the map; the object of the whole investigation is to express r in terms of θ. Let the length of a small rectangle on the earth be $\delta\theta$, the corresponding length on the map δr. Also let ϕ be the minute angle of azimuth under which, in both cases, the breadth of the rectangle is seen from the Centre of Reference or the centre of the map. Then we have

$$l = \delta\theta, \quad l+\delta l = \delta r, \quad \delta l = \delta r - \delta\theta;$$
$$b = \phi . \sin\theta, \quad b+\delta b = \phi . r, \quad \delta b = \phi . (r - \sin\theta);$$
$$\left(\frac{\delta l}{l}\right)^2 + \left(\frac{\delta b}{b}\right)^2 = \left(\frac{\delta r}{\delta\theta} - 1\right)^2 + \left(\frac{r}{\sin\theta} - 1\right)^2.$$

This quantity expresses the evil on each small rectangle. The

product of the evil by the extent of surface which it affects, omitting the general multiplier ϕ, is

$$\left\{\left(\frac{\delta r}{\delta\theta}-1\right)^2+\left(\frac{r}{\sin\theta}-1\right)^2\right\}\times\sin\theta\,.\,\delta\theta.$$

Consequently the summation of the partial evils for the whole map is represented by

$$\int d\theta\,.\left\{\left(\frac{dr}{d\theta}-1\right)^2+\left(\frac{r}{\sin\theta}-1\right)^2\right\}\times\sin\theta.$$

Or if $r-\theta=y$, and if we put p for $\frac{dy}{d\theta}$, the expression is

$$\int d\theta\,.\left\{p^2\sin\theta+\frac{(y+\theta-\sin\theta)^2}{\sin\theta}\right\};$$

and this integral, through the surface to which the map applies, is to be minimum.

7. This is a case of the Calculus of Variations. The function V to be integrated exhibits values for the differential coefficients

$$\mathrm{M}=\frac{d\mathrm{V}}{d\theta},\quad \mathrm{N}=\frac{d\mathrm{V}}{dy},\quad \mathrm{P}=\frac{d\mathrm{V}}{dp}.$$

The equation of solution is $\mathrm{N}-\frac{d(\mathrm{P})}{d\theta}=0$. Now in consequence of the existence of a value for M, we cannot adopt the facilities of solution which present themselves when $\mathrm{M}=0$, and we must therefore take the equation $\mathrm{N}-\frac{d(\mathrm{P})}{d\theta}=0$ without modification. Here

$$\mathrm{N}=\frac{2(y+\theta-\sin\theta)}{\sin\theta},\quad \mathrm{P}=2p\sin\theta,\quad \frac{d(\mathrm{P})}{d\theta}=2q\sin\theta+2p\cos\theta;$$

and the equation $\mathrm{N}-\frac{d(\mathrm{P})}{d\theta}$ becomes

$$\frac{y+\theta-\sin\theta}{\sin\theta}-\sin\theta\,.\,\frac{d^2y}{d\theta^2}-\cos\theta\,.\,\frac{dy}{d\theta}=0;$$

or

$$\sin^2\theta\,.\,\frac{d^2y}{d\theta^2}+\sin\theta\,.\cos\theta.\frac{dy}{d\theta}-y=\theta-\sin\theta.$$

8. For $\theta-\sin\theta$ put the more general symbol Θ. To solve the equation, assume $z=\sin\theta\,.\,\frac{dy}{d\theta}+y$. Then, by actual differentiation and substitution,

$$\sin\theta \cdot \frac{dz}{d\theta} - z = \sin^2\theta \cdot \frac{d^2y}{d\theta^2} + \sin\theta \cdot \cos\theta \cdot \frac{dy}{d\theta} - y,$$

or

$$\sin\theta \cdot \frac{dz}{d\theta} - z = \Theta.$$

This equation is integrable when multiplied by $\frac{1}{\sin^2\frac{\theta}{2}}$; the solution gives

$$z = \frac{1}{2}\tan\frac{\theta}{2} \cdot \int d\theta \cdot \frac{\Theta}{\sin^2\frac{\theta}{2}}.$$

Therefore

$$\sin\theta \cdot \frac{dy}{d\theta} + y = \frac{1}{2}\tan\frac{\theta}{2} \cdot \int d\theta \cdot \frac{\Theta}{\sin^2\frac{\theta}{2}}.$$

This equation is integrable when multiplied by $\frac{1}{\cos^2\frac{\theta}{2}}$; the solution gives

$$y = \frac{1}{4} \cdot \operatorname{cotan}\frac{\theta}{2} \cdot \int d\theta \cdot \frac{\sin\frac{\theta}{2}}{\cos^3\frac{\theta}{2}} \cdot \int d\theta \cdot \frac{\Theta}{\sin^2\frac{\theta}{2}}.$$

If $\frac{\theta}{2} = \psi$, the solution may be put in the form

$$y = \cot\psi \cdot \int d\psi \cdot \frac{\sin\psi}{\cos^3\psi} \cdot \int d\psi \cdot \frac{\Theta}{\sin^2\psi};$$

or

$$y = \frac{1}{2} \cdot \frac{1}{\sin\psi \cdot \cos\psi} \cdot \int d\psi \cdot \frac{\Theta}{\sin^2\psi} - \frac{1}{2}\cot\psi \cdot \int d\psi \cdot \frac{\Theta}{\sin^2\psi \cdot \cos^2\psi};$$

or

$$y = \frac{1}{2}\tan\psi \cdot \int d\psi \cdot \frac{\Theta}{\sin^2\psi} - \frac{1}{2}\cot\psi \cdot \int d\psi \cdot \frac{\Theta}{\cos^2\psi}.$$

9. If we substitute for Θ our value $\theta - \sin\theta$, we obtain

$$y = -\theta - 2\cot\frac{\theta}{2} \cdot \log\cos\frac{\theta}{2} + C' \cdot \operatorname{cotan}\frac{\theta}{2} + C'' \cdot \frac{1}{\sin\theta}$$

(in which we may vary the arbitrary terms, remarking that

$$\frac{1}{\sin\theta} = \frac{1}{2}\tan\frac{\theta}{2} + \frac{1}{2}\cot\frac{\theta}{2}\Big);$$

and if we determine the arbitrary constants so that, when $\theta = 0$,

y shall $=0$ and $\frac{dy}{d\theta}$ shall $=0$ (that is, so that the central parts of the map shall correspond exactly with the region about the Centre of Reference),

$$y=\tan\frac{\theta}{2}-\theta-2\cot\frac{\theta}{2}.\log\cos\frac{\theta}{2},$$

and

$$r=\theta+y=\tan\frac{\theta}{2}+2\cot\frac{\theta}{2}.\log\sec\frac{\theta}{2},$$

in which the logarithm is the Napierian or hyperbolic logarithm.

This equation entirely defines the nature of the Projection by Balance of Errors. The numerical values of r, for a series of values of θ, will shortly be given in a tabular form.

10. In order to obtain a numerical estimate of the two errors of a Projection, we must make use of the formulæ,

$$\frac{\text{projected area}}{\text{original area}}=\frac{r}{\sin\theta}.\frac{dr}{d\theta};$$

$$\frac{\text{projected breadth}}{\text{projected length}}\times\frac{\text{original length}}{\text{original breadth}}=\frac{r}{\sin\theta}.\frac{1}{\frac{dr}{d\theta}};$$

and for all the projections which we desire to compare, we must express r and $\frac{dr}{d\theta}$ in terms of θ, and must substitute in these formulæ.

11. The Projections which I shall compare are the following (in the formulæ, ψ is put for $\frac{\theta}{2}$):—

(1) The Projection with Equal Radial Degrees. In this,

$$r=\theta,\quad \frac{p.a.}{o.a.}=\frac{\theta}{\sin\theta},\quad \frac{p.b.}{p.l.}\times\frac{o.l.}{o.b.}=\frac{\theta}{\sin\theta}.$$

(2) The Projection with Unchanged Areas. In this,

$$r=2\sin\psi,\quad \frac{p.a.}{o.a.}=1,\quad \frac{p.b.}{p.l.}\times\frac{o.l.}{o.b.}=\sec^2\psi.$$

(3) The Stereographic Projection. In this,

$$r=2\tan\psi,\quad \frac{p.a.}{o.a.}=\sec^4\psi,\quad \frac{p.b.}{p.l.}\times\frac{o.l.}{o.b.}=1.$$

(4) Sir H. James's Projection. Here

$$r=\frac{5\sin\theta}{3+2\cos\theta},\quad \frac{p.a.}{o.a.}=\frac{25(3\cos\theta+2)}{(3+2\cos\theta)^3},\quad \frac{p.b.}{p.l.}\times\frac{o.l.}{o.b.}=\frac{3+2\cos\theta}{3\cos\theta+2}.$$

This projection fails when $\cos\theta=-\frac{2}{3}$, or $\theta=131^\circ\ 49'$.

(5) The Projection by Balance of Errors. Here

$$r = \tan\psi + 2\cot\psi \,.\, \log\sec\psi,$$

$$\frac{p.a.}{o.a.} = \left(\frac{1}{2}\,.\,\sec^2\psi + \operatorname{cosec}^2\psi\,.\,\log\sec\psi\right)$$

$$\div\left(1 + \frac{1}{2}\sec^2\psi - \operatorname{cosec}^2\psi\,.\,\log\sec\psi\right),$$

$$\frac{p.b.}{p.l.} \times \frac{o.l.}{o.b.} = \frac{\sec^2\psi + 2\operatorname{cosec}^2\psi\,.\,\log\sec\psi}{2 + \sec^2\psi - 2\operatorname{cosec}^2\psi\,.\,\log\sec\psi}.$$

From these formulæ the numbers in the following Tables are computed:—

12. Table of Radial Distances from the Centre of the Map, for different Great-Circle Distances θ from the Centre of Reference.

θ.	Equal Radial Degrees.	Unchanged Areas.	Stereographic.	Sir H. James.	Balance of Errors.
5°	0·08727	0·08724	0·08732	0·08729	0·08728
10	0·17453	0·17431	0·17498	0·17471	0·17465
15	0·26180	0·26105	0·26331	0·26240	0·26218
20	0·34907	0·34730	0·35265	0·35047	0·34997
25	0·43634	0·43288	0·44339	0·43907	0·43811
30	0·52360	0·51764	0·53590	0·52831	0·52672
35	0·61087	0·60141	0·63060	0·61830	0·61589
40	0·69814	0·68404	0·72794	0·70915	0·70577
45	0·78540	0·76537	0·82843	0·80094	0·79650
50	0·87267	0·84524	0·93262	0·89375	0·88825
55	0·95994	0·92350	1·04113	0·98761	0·98121
60	1·04720	1·00000	1·15470	1·08253	1·07563
65	1·13447	1·07460	1·27414	1·17849	1·17178
70	1·22174	1·14715	1·40042	1·27535	1·27000
75	1·30901	1·21752	1·53465	1·37299	1·37068
80	1·39628	1·28558	1·67820	1·47105	1·47434
85	1·48354	1·35118	1·83266	1·56915	1·58157
90	1·57080	1·41421	2·00000	1·66666	1·69315
95	1·65807	1·47455	2·18262	1·76275	1·81002
100	1·74534	1·53209	2·38351	1·85623	1·93342
105	1·83261	1·58671	2·60645	1·94558	2·06492
110	1·91988	1·63830	2·85630	2·02873	2·20659
115	2·00714	1·68678	3·13937	2·10303	2·36118
120	2·09440	1·73205	3·46110	2·16506	2·53243
125	2·18167	1·77402	3·84196	2·21052	2·72550
130	2·26894	1·81262	4·28901	2·23412	2·94776
135	2·35620	1·84776	4·82843	After this	3·20996
140	2·44347	1·87939	5·49495	the radius	3·52847
145	2·53074	1·90743	6·34319	diminishes.	3·92934
150	2·61801	1·93185	7·46410		4·44831
155	2·70528	1·95259	9·02142		5·18929
160	2·79255	1·96962	11·34256		6·28868

13. Table of Exaggeration, as shown by the Proportions of Projected Area to Original Area, for different Great-Circle Distances θ from the Centre of Reference.

θ.	Equal Radial Degrees.	Unchanged Areas.	Stereographic.	Sir H. James.	Balance of Errors.
° 5	1·00127	1·00000	1·00382	1·00229	1·00191
10	1·00508	1·	1·01537	1·00917	1·00767
15	1·01152	1·	1·03496	1·02073	1·01735
20	1·02060	1·	1·06315	1·03706	1·03127
25	1·03245	1·	1·10071	1·05835	1·04961
30	1·04544	1·	1·14875	1·08485	1·07278
35	1·06501	1·	1·20871	1·11674	1·10131
40	1·08610	1·	1·28250	1·15432	1·13585
45	1·11072	1·	1·37255	1·19789	1·17728
50	1·13919	1·	1·48217	1·24774	1·22668
55	1·17186	1·	1·61542	1·30412	1·28549
60	1·20920	1·	1·77778	1·36719	1·35543
65	1·25174	1·	1·97644	1·43692	1·43894
70	1·30014	1·	2·22097	1·51302	1·53909
75	1·35517	1·	2·52426	1·59470	1·65992
80	1·41780	1·	2·90391	1·68043	1·80697
85	1·48920	1·	3·38436	1·76759	1·98777
90	1·57080	1·	4·00000	1·85185	2·21269
95	1·66439	1·	4·80028	1·92641	2·49650
100	1·77225	1·	5·85774	1·98088	2·86051
105	1·89724	1·	7·28135	1·99969	3·33627
110	2·04307	1·	9·23921	1·96010	3·97176
115	2·21462	1·	11·99861	1·82952	4·84226
120	2·41840	1·	16·00000	1·56250	6·05133
125	2·66332	1·	21·99771	1·09761	7·86206
130	2·96188	1·	31·34779	0·35540	10·58547
135	3·33216	1·	46·62740	After this	14·89565
140	3·80135	1·	73·07911	the projec-	22·28290
145	4·41219	1·	122·30176	tion fails.	35·68061
150	5·23598	1·	254·44946		70·86947
155	6·40119	1·	455·67252		123·61888
160	8·16480	1·00000	1099·81373		290·08199

14. Table of Distortion, as shown by the Proportions of the Transverse Side to the Radial Side, in the projection of an area originally square, for different Great-Circle Distances θ from the Centre of Reference.

θ.	Equal Radial Degrees.	Unchanged Areas.	Stereographic.	Sir H. James.	Balance of Errors.
° 5	1·00127	1·00191	1·00000	1·00076	1·00084
10	1·00508	1·00765	1·	1·00307	1·00382
15	1·01152	1·01733	1·	1·00696	1·00861
20	1·02060	1·03109	1·	1·01252	1·01526
25	1·03245	1·04915	1·	1·01986	1·02386
30	1·04544	1·07180	1·	1·02914	1·03444
35	1·06501	1·09941	1·	1·04057	1·04693
40	1·08610	1·13247	1·00000	1·05443	1·06137

Table (*continued*).

θ.	Equal Radial Degrees.	Unchanged Areas.	Stereographic	Sir H. James.	Balance of Errors.
45°	1·11072	1·17157	1·00000	1·07107	1·07775
50	1·13919	1·21744	1·	1·09094	1 09604
55	1·17186	1·27099	1·	1·11461	1·11617
60	1·20920	1·33333	1·	1·14286	1·13812
65	1·25174	1·40586	1·	1·17668	1·16171
70	1·30014	1·49029	1·	1·21744	1·18679
75	1·35517	1·58879	1·	1·26695	1·21311
80	1·41780	1·70409	1·	1·32780	1·24033
85	1·48920	1·83966	1·	1·40365	1·26801
90	1·57080	2·00000	1·	1·50000	1·29559
95	1·66439	2·19095	1·	1·62533	1·32235
100	1·77225	2·42028	1·	1·79351	1·34743
105	1·89724	2·69840	1·	2·02883	1·36980
110	2·04307	3·03961	1·	2·37793	1·38832
115	2·21462	3·46391	1·	2·94308	1·40171
120	2·41840	4·00000	1·	4·00000	1·43395
125	2·66332	4·69017	1·	6·63459	1·40808
130	2·96188	5·59891	1·	23 93204	1·39883
135	3·33216	6·82842	1·	After this	1·38347
140	3·80135	8·54863	1·	the projec-	1·35228
145	4·41219	11·05901	1·	tion fails.	1·31529
150	5·23598	15·95147	1·		1·26275
155	6·40119	21·34648	1·		1·21965
160	8·16480	33·16545	1·00000		1·16546

15. The last two Tables, which enable us to compare numerically the Exaggeration and the Distortion in the different systems, will give us the means of comparing the systems generally. I shall make this comparison for the two values of θ, 115°, and 135°: the former, because it is nearly the extreme value of θ in Sir H. James's maps; the latter because, while on the one hand it may be sometimes desirable, on the other hand it is the largest that is likely ever to be wanted.

16. For $\theta = 115°$, the Exaggeration of the stereographic system is 12·0; this will perhaps be judged so large as to exclude it from further consideration. The merits of the others stand in the order,—

1. Unchanged Areas.
2. Sir H. James's.
3. Equal Radial Degrees.
4. Balance of Errors.

But when we consider the Distortion, the order is,—

1. Balance of Errors.
2. Equal Radial Degrees.
3. Sir H. James's.
4. Unchanged Areas.

The distortion in Sir H. James's is expressed by 2·9; and this, in my opinion, is a more serious inconvenience than the exaggeration in the Balance of Errors, namely 4·8. On the whole, I think that, for this value of θ, the Balance of Errors is preferable to Sir H. James's. The nearest in merit, I think, is that of Equal Radial Degrees; but the distortion of Balance of Errors 1·4 is preferable to that of Equal Radial Degrees 2·2, while the exaggeration 4·8 is not much more injurious than 2·2. I prefer the Balance of Errors.

17. For $\theta = 135°$, Sir H. James's is inapplicable. The remaining systems stand in these orders:—

Exaggeration.		Distortion.	
1. Unchanged Areas . .	1·0	1. Balance of Errors .	1·4
2. Equal Radial Degrees .	3·3	2. Equal Radial Degrees	3·3
3. Balance of Errors . .	14·9	3. Unchanged Areas .	6·8

In my opinion, the Balance of Errors is here the best. A square whose sides are 1, 1, is projected into a parallelogram whose sides are 3·3, 4·6; and this is better than the parallelogram in the Equal Radial Degrees whose sides are 1, 3·3. This is on the supposition that we desire to preserve an intelligible representation of every part of the earth depicted in the map. I will shortly state under what circumstances this opinion may perhaps be modified.

18. If we take for Centre of Reference the point defined by longitude $3^h\ 30^m$ east of Greenwich, latitude 23° north, the circle whose radius is 135° will contain every continent and large island, including Australia and New Zealand, omitting only the South Pacific Ocean. If we take for Centre of Reference the point defined by longitude 1^h east of Greenwich, latitude 10° south, the circle whose radius is 135° will contain every continent and large island as before, omitting only the North Pacific Ocean. In such maps, the countries which are found on the borders of the map are sufficiently extensive and important to require to be exhibited without much distortion; and all constructions are equally troublesome. For these maps, therefore, I should use the system of Balance of Errors.

19. If, however, we take for Centre of Reference the point defined by longitude $16^h\ 20^m$ east of Greenwich, latitude 77° north, the circle whose radius is 135° will contain the same continents and large islands as before, including also the small islands of the Pacific Ocean, omitting only the Antarctic Seas. Such a map is of extraordinary value, because it not only contains all the known lands, but may also exhibit all the sea-courses between the southern capes. But where, as in this case, the boundary is touched by little more than the headlands, distortion is less im-

portant, and the objections to the system of Equal Radial Degrees are much diminished. And, as the Centre of Reference is so near to the north pole, no serious discordance, probably no perceptible discordance, will be produced, if we describe the Parallels as circles whose centres are in the north pole and whose radii increase by equal degrees, and the meridians by straight radii from the north pole with equal angles between each radius and the proximate radius; and if we afterwards limit the map by a boundary-circle whose centre is at longitude $16^h\ 20^m$ east of Greenwich, latitude 77° north. This construction would be extremely easy.

20. Reverting now to the general theory, it appears that while the Stereographic Projection, in which $r=2\tan\frac{\theta}{2}$, possesses the very great merit of being free from distortion in its small elements, yet a more acceptable map is given by advancing in some measure towards the Projection by Equal Radial Degrees, in which $r=\theta=\infty\times\tan\frac{\theta}{\infty}$. It is evident that this may be done conveniently by using larger numbers instead of the 2 and 2 which occur in the stereographic formula. Thus we may conveniently use $r=3\tan\frac{\theta}{3}$, in which, Exaggeration $=\frac{3}{\sin\theta}\cdot\tan\frac{\theta}{3}\cdot\sec^2\frac{\theta}{3}$, Distortion $=\frac{3}{\sin\theta}\cdot\tan\frac{\theta}{3}\cdot\cos^2\frac{\theta}{3}$. Or $r=4\tan\frac{\theta}{4}$, which gives,

$$\text{Exaggeration}=\frac{4}{\sin\theta}\tan\frac{\theta}{4}\cdot\sec^2\frac{\theta}{4}=\sec\frac{\theta}{2}\cdot\sec^4\frac{\theta}{4},$$

$$\text{Distortion}=\frac{4}{\sin\theta}\cdot\tan\frac{\theta}{4}\cdot\cos^2\frac{\theta}{4}=\sec\frac{\theta}{2}.$$

In either of these the Exaggeration is diminished, and Distortion is introduced, but more in the second than in the first.

21. I will now allude to the process by which any of these Projections can be adapted to any Point of Reference whatever. The process is in fact a transfer from one system of projection to another system of projection, and is founded upon this theorem: that if in one projection we describe a series of Circles whose common centre is the Centre of the Map (corresponding to the Point of Reference) having radii equal to values of r corresponding on that projection to values of θ which increase by uniform quantities as 5° or 10°, and if we draw from that centre Radial Lines at equal angles of azimuth; and if we do the same thing for another projection; then all the intersections of Meridians and Parallels referred to the pole of the earth will occupy on one projection the same places, in reference to the circles and radial lines above-mentioned, which they occupy on the other projection.

Thus, if we possess a map in which the meridians and parallels are drawn through the circles and radial lines on one projection, then for any other projection we have merely to draw radial lines, and to describe circles with the radii given by the Table of article 12, by the formulæ of article 20, or by equivalent statements, for that other projection; and we can at once lay down among the radii and circles of the second projection the intersections corresponding to those of the first projection as seen among *its* radii and circles.

22. There is one projection in which the Meridians and Parallels are described with comparative facility, because all are accurately circular arcs, namely the Stereographic. This projection, therefore, will be most proper for use as the standard projection, by means of which any others may be drawn. As a termination to this paper, I will here place the formulæ required for drawing a Stereographic Map with any Centre of Reference.

Let a be the linear radius of the circle which would include a hemisphere of the earth, β the radius of the proposed map, in degrees. Let the Centre of Reference be in north latitude α. (If in south latitude, it will only be necessary to invert the map.)

(1) The linear radius of the entire map will be $a \cdot \tan\frac{\beta}{2}$.

(2) Through the centre of the map a line must be drawn as polar axis. On this line will lie the centres of all the circles representing parallels of latitude.

(3) Let x be the north latitude of any parallel which is to be drawn (x being treated as an algebraically negative quantity for parallels in south latitude). One intersection of the circle representing this parallel, with the polar axis, will be north of the centre of the map by $a \cdot \tan\frac{x-\alpha}{2}$; the other intersection will be north of the centre by $a \cdot \operatorname{cotan}\frac{x+\alpha}{2}$. The centre of the circle will be north of the central point by half the sum of these quantities, or by

$$\frac{a}{2} \cdot \cos\alpha \cdot \sec\frac{x-\alpha}{2} \cdot \operatorname{cosec}\frac{x+\alpha}{2}.$$

The radius of the circle will be half the difference of these quantities, or

$$\frac{a}{2} \cdot \cos x \cdot \sec\frac{x-\alpha}{2} \cdot \operatorname{cosec}\frac{x+\alpha}{2}.$$

The rules of algebraic signs are to be severely followed.

(4) The north pole is north of the central point by

$a \cdot \tan\left(45° - \frac{\alpha}{2}\right)$, and the south pole is south of the central point by $a \cdot \tan\left(45° + \frac{\alpha}{2}\right)$, or $a \cdot \operatorname{cotan}\left(45° - \frac{\alpha}{2}\right)$.

(5) Bisect the line joining the poles (the point of bisection will be south of the central point by $a \cdot \tan \alpha$), and through the bisecting point draw an indefinite straight line at right angles to the polar axis. (This line represents the parallel for south latitude α.) On this transversal line will lie the centres of all the circles representing meridians.

(6) Let y be the angular measure of longitude east of the Point of Reference, of any meridian which is to be drawn. The eastern intersection, of the circle representing this meridian, with the transversal line, will be at the distance $a \cdot \text{secant}\, \alpha \cdot \tan \frac{y}{2}$ from the polar axis; and the western intersection will be at the distance $a \cdot \text{secant}\, \alpha \cdot \operatorname{cotan} \frac{y}{2}$. The western distance of the centre of the circle will be $a \cdot \text{secant}\, \alpha \cdot \operatorname{cotan} y$, and the radius of the circle will be $a \cdot \text{secant}\, \alpha \cdot \text{cosecant}\, y$.

When, by means of these formulæ, the circles for Parallels and Meridians have been drawn, then concentric circles are to be described having the centre of the map for their centre, and with radii successively equal to $a \cdot \tan \frac{10°}{2}$, $a \cdot \tan \frac{20°}{2}$, &c., representing great-circle distances 10°, 20°, &c., from the Point of Reference; and radii are to be drawn at equal azimuthal angles. Then the map may be used for laying down the intersections of meridians and parallels (as described in article 21) for any projection whatever, with the same Point of Reference.

Royal Observatory, Greenwich,
September 25, 1861.

LIV. *On the Deficiency of Rain in an elevated Rain-gauge, as caused by Wind.* *By* W. S. Jevons, *B.A. of University College, London**.

[With a Plate.]

1. FROM the year 1767 many meteorologists have held the settled opinion that the larger part of the rain which falls upon the surface of the earth does not proceed from the clouds, as we should naturally suppose, but is derived from the lower strata of the atmosphere, within 200 or 300 feet of the

* Communicated by the Author, having been partly read at the meeting of the British Association at Manchester, September 1861.

surface. This paradox has been founded upon the fact that a rain-gauge, when placed at a moderate elevation in the atmosphere, is found to collect much less rain than if placed upon the ground. As the sudden increase of rain while it falls through the intervening air cannot be explained in accordance with the known laws of nature, many writers have spoken doubtfully of this subject, but have brought forward only scanty or palpably erroneous arguments to account for the experimental results.

2. I now hope to show that the observed differences of rainfall must be attributed to the influence of the wind upon our mode of experiment.

3. In observations with rain-gauges at different elevations, the higher gauges have been placed upon the roofs of houses, the summits of church-towers, or other erections which act as obstacles to the wind. It is obvious, too, that the rain-gauge is itself an obstacle, causing the wind to swerve aside, and to change the direction in which the rain-drops fall.

4. In order to determine the curves which the wind describes in meeting such obstacles, I have performed some small experiments. A vessel is formed of two oblong plates of glass, enclosing a layer of air about a quarter of an inch thick. One end of the vessel communicates through a pipe with a chimney or an aspirator, so that a regulated current of air may be drawn through it, to represent on a small scale a section of the wind moving over the surface of the earth. The curves described by the currents of air are shown very distinctly and beautifully by simply holding a piece of smoking brown paper in the draught of air which is about to enter the glass vessel. We may now place in the lower part of the current any small obstacle to represent a house or a rain-gauge placed in wind, and the curves described by the air will be depicted by the streams of smoke.

In trying such experiments, it is soon perceived that the curves are similar so long as the velocity of the current changes proportionally to the magnitude of the obstacle; and I am led to believe that the miniature experiment will indicate the course pursued by the actual wind meeting an obstacle, provided that the velocity of the wind and the magnitude of the obstacle bear somewhat the same proportion to each other as in the experiment. From such observations I have drawn the dotted lines in figs. 2 and 3, Pl. VI. They are intended to represent the course pursued by horizontal strata of air meeting an obstacle, such as a house (fig. 2), or a rain-gauge (fig. 3). Whatever may be the value of the experimental method, it cannot be denied that the air must move somewhat as shown in these figures.

5. A stream of air, then, meeting an obstacle leaps over it;

in so doing it is forced against the adjoining parallel stream of air, which must also diverge from the straight direction, and similarly impinge upon the next stream. But the increased pressure produced by the impact causes the streams of air to move more rapidly, and to diminish in thickness at the same time; and the disturbance of the streams of air will cease at the point where the total decrease of size of the streams is equal to the height of the obstacle. It is at least obvious that when a uniform wind meets an obstacle, some parts of the air must move more rapidly, just as a river moves most rapidly in the narrowest parts of its channel. It is quite in accordance, too, with our common experience, that an obstacle increases the velocity and force of the wind; thus the wind is always most fierce at the corner of a house, the end of a wall, or the summit of a hill.

6. We now have the whole explanation of the rain-observations in question. A drop of rain in falling is influenced at once by gravity and the motion of the air. It describes the diagonal of a rectangle, of which the perpendicular side represents the falling velocity of the drop, and the horizontal side the velocity communicated by the wind. In other words, we may say that the tangent of the angle of inclination (from the vertical direction) of the path of the falling drop varies nearly as the velocity of the wind.

Now conceive two equal drops of rain falling into a current of air at points where the velocity is not the same. They will not pursue parallel paths, but the one drop will either approach to, or recede from the other. The effect will be to increase or diminish the quantity of rain falling in the intermediate space.

To show clearly the nature of this effect, we may imagine the stream of air A B in Plate VI. fig. 1, to be suddenly contracted at C D to half its previous thickness, so that of course it must there commence to move with double velocity. At E F the stream dilates to its original size, and of course recovers its first velocity. The course of equidistant rain-drops falling into wind under such imaginary circumstances would be represented by the oblique black lines, and it is obvious that less rain would fall in the windward part of the contracted space than elsewhere.

7. To represent a real shower of rain falling upon an obstacle, we have only to conceive the drops of rain as falling through a great number of strata, all varying in velocity and thickness. I have thus conjecturally drawn the full lines in fig. 2 to represent the paths of the rain-drops in a shower falling through wind upon an obstacle such as a house, or tower which bears upon its summit an ordinary rain-gauge. In fig. 3, which is drawn upon a much different scale, the rain-gauge is the only obstacle, being

supposed fixed in mid-air. It is here, I venture to hope, rendered quite plain that *less rain will fall upon the summit of the obstacle than elsewhere, the surplus being carried forward to the lee side of the obstacle.* I entertain no doubt that we have in this process a sufficient explanation of the observed deficiency of rain in elevated places.

8. It is an evident corollary of this explanation, that no deficiency of rain would be observed did the measuring instrument cause no disturbance in the wind. But only a gauge of which the mouth is level with the ground fulfils this condition. Probably, indeed, the church-tower or house upon which a gauge is usually elevated occasions the chief part of the deficiency. Hence a gauge suspended in mid-air would collect more rain than if it were placed on a house. Yet a rain-gauge is itself an obstacle of some importance, and will cause a part of the rain to pass on unmeasured, as I have attempted to represent in fig. 3. The hollow of the funnel in this figure, it will be observed, is filled up with an *eddy* of wind.

9. In these drawings, I should observe, some little exaggeration must be excused: no notice, too, is taken of the motion of the wind in the third dimension of space, that is, round the obstacle instead of over it.

10. Thus having reason to suppose that the deficiency of rain at elevated points was due to the disturbance of the wind, I have examined all the observations and statements I could meet with bearing on the subject, and find my opinion, on the whole, strongly confirmed.

An intelligent observer, Mr. H. Boase of Penzance, after four months' experiment, remarks*, "Having observed that the difference between the first and the other gauges varied with more or less wind, its velocity has been registered from observation; but not having an accurate anemometer, we cannot yet offer any certain conclusion further than this, that the difference of the quantity of rain received in a gauge placed on the top of a building, and one at a level with the surface of the ground, is, for some reason or other, *proportional to the velocity of the wind.*"

11. Again, taking the measurements of rain† made by Luke Howard, and arranging them in the order of the ratio of the quantities in the lower and higher gauges, we find that we have also arranged them almost exactly in the order of the amount of accompanying wind, as indicated by the notes annexed. The results are as follows:—

* Annals of Philosophy (July 1822), new series, vol. iv. p. 18.
† Howard's 'Climate of London,' vol. ii. p. 158.

Ratio of rain in lower gauge to that in upper gauge.	Howard's remarks (in full).
3·00	Windy night; nimbus at sunset.
2·78	Stormy A.M.; wet P.M.
2·33	Cloudy; much wind; stormy night.
2·20	Much cloud with a fresh breeze.
2·00	Windy night.
1·75	Three currents in the air.
1·61	Showery day; cirrostratus evening.
1·60	Misty rain about midday; little wind veering from S.W. to E.
1·19	Cloudy; drizzling.
1·17	Rain by night.
1·17	(No remark.)
1·11	Showers chiefly by night.
1·10	Rain by night.
1·10	(No remark.)
1·08	Rain by night.
1·00	Clear A.M. with dew; nimbi; vane S.E. P.M., a heavy shower to S.; wind veered by S. to N.W.; then much cloud and rain.
1·00	Showers.

12. At the Greenwich Observatory, measurements of rain from three gauges placed at different heights have been daily recorded for about twenty years past. Examining the individual results, I was surprised to find great irregularity and want of accordance. Thus several hundredths of an inch of rain are often registered in the lowest gauge and *none* in the highest. Occasionally the middle gauge alone has caught any rain! The following will serve as a rather extreme specimen of these discordances:—

1844. Jan. and Feb.	30th. inch.	31st. inch.	2nd. inch.	6th. inch.	7th. inch.	9th. inch.
Highest gauge	·01	·02	·08	·08	·21	·00
Middle „	·01	·00	·19	·17	·11	·33
Lowest „	·01	·04	·21	·18	·23	·16

These observations having been made by gentlemen of high ability and well-known scientific zeal, the discordances can only be attributed to the erroneous nature of the rain-gauge, and to the very unsuitable position and form of the Greenwich Observatory for rain observations: of course it is useless to look for any uniform law or ratio where such discrepancies may occur. The discordances, too, have no obvious relation with the force of the

wind, but might perhaps be explained by comparison with the direction and force of the wind combined. They serve me here amply to establish the unsatisfactory nature of the best rain measurements.

13. It is in this subject quite fallacious to appeal to *average results*; for an appearance of uniformity and law will arise in the long run, according to the doctrine of probabilities, however irregular and various the causes which produce the difference. A law of nature must appear in every case in which it acts alone, reasonable error of observation being allowed for; but the discrepancies of individual rain observations at different altitudes are such as can come under no law. Even average or total quantities for short periods are extremely discordant. Prof. Phillips's observations are stated in weekly totals*; but in the week February 19 to 26, 1832, we find that the lowest gauge received nearly six times as much rain as the upper one upon the York Minster, while in the next succeeding week but one the lower gauge contained only 1·22 times (or $1\frac{1}{4}$) as much as the other. The circumstances fully explain this difference, "violent gales" having occurred in the former week and "perpendicular rain, without a trace of wind, in large drops" in the latter. This last statement will again be referred to (see par. 27).

14. Arago's results at the Paris Observatory, although pretty uniform when stated in yearly averages, exhibit similar discordances in the separate months. From a Table in the *Encyclopædia Metropolitana*, Art. Meteorology (p. 115), I extract the results of the following three months, being the

Difference in Centimetres between results of Higher and Lower Gauges at the Paris Observatory.

	1826.	1827.	1828.	1829.
March . . .	·285	1·207	·790	·174
May	·430	1·575	·210	·010
December . .	·810	1·220	·190	·030

Here in the same month, May, the difference varies from $\frac{1}{100}$ to $\frac{157}{100}$ (centimetre)!

15. The deficiency of rain in an elevated rain-gauge varies greatly according to the season of the year; and on an average the greatest deficiency is found during the winter. It is a phenomenon of a *wintry character*, observes Prof. Phillips†. But of all the months March generally shows the largest deficiency‡; and Prof. Phillips,

* Brit. Assoc. Report, 1833, Trans. Sections, p. 403.

† Brit. Assoc. Report, 1834, Trans. Sections, p. 562. See also Howard's 'Climate of London,' vol. i. p. 104; and Schouw, *Climat d'Italie*, p. 135: "qu'elle est beaucoup plus forte en hiver qu'en été."

‡ See the observations of Dr. Heberden, Phil. Trans. vol. lix. (1769)

in discussing his observations*, adds the significant remark, "March very anomalous." Now March is in Europe the month in which strong, dry, north-east winds and equinoctial gales most occur, the very circumstances under which we should expect the results to be most erroneous.

16. I may lastly mention the observations of Dr. Buist, who having made four simultaneous measurements in the Island of Bombay, to determine the fall of rain at different heights below 200 feet, reported to the British Association, in 1852, that the results were entirely discordant. Although all proper precautions were taken†, "no satisfactory conclusion could be drawn, because the gauges at the several heights below and at 200 feet did not give uniform results,—sometimes the most elevated gauges having the greatest fall of rain, and at other times the lower. Nor did gauges at similar heights receive the same quantity of rain."

17. Although the effect of an obstacle upon the wind as causing a separation or approximation of the rain-drops, and a deficiency of rain in an elevated gauge, has now, I believe, for the first time been distinctly brought forward, several writers have made suggestions nearly to the same effect. Thus Howard speaks‡ of strong winds as robbing the higher gauge. Dr. Trail says§ of Prof. Phillips's observations, "These differences are too considerable to be attributed to anything but some imperfection in the instrument when much exposed to gales of wind; and it probably arises from eddies being formed round the rim of the funnel, which divert part of the water."

Again, H. Meikle writes in the 'Annals of Philosophy||,' "I can hardly pretend to give a complete solution of this well-known paradox, but am disposed to think it is in some way owing to the obstruction which the gauge itself offers to the wind. Perhaps the winds being made to rush with greater rapidity, and a little upward in beginning to pass over the mouth of the gauge, prevents the rain from falling into that part of it which is next the wind."

This almost coincides with my own explanation; but the remark is confined to the operation of the rain-gauge, which is usually an inconsiderable obstacle compared with the house or tower upon which the gauge is placed.

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p. 359. Those by Bugge at Copenhagen, *Mém. de l'Acad. de Copenhagen*, nouv. sér. vol. v. p. 227; or in Schouw, *Climat d'Italie*, p. 131. In Arago's observations February is slightly more deficient than March.

* Brit. Assoc. Report, 1833, Trans. Sections, p. 408.

† Brit. Assoc. Report, 1852, Trans. Sections, p. 25.

‡ Climate of London, vol. i. p. 104.

§ Physical Geography (7th ed.), *Encyc. Brit.* reprint, p. 184.

|| Vol. xiv. p. 312, for the year 1819.

Prof. Bache's "Note on the Effect of Deflected Currents of Air on the quantity of Rain collected by a Rain-gauge," communicated to the British Association in 1838, is to a different effect. It proves experimentally the immense differences which may occur between gauges placed at the different angles of a building, but does not show why a gauge on the top of an obstacle must on an average suffer a loss of rain. He found, however, that the gauges to the leeward received in general more rain than those to the windward, a fact fully in accordance with my theory.

18. It is hardly necessary to add that my explanation has no connexion with that of M. Flaugergues*, who in an unfortunate moment mistook the sine for the radius of an angle, and argued that "less rain will fall into the horizontal opening of the rain-gauge when the rain is inclined than if it fell vertically, or in a direction less inclined." As long as the drops fall in parallel paths no such effect can be produced; it is the divergence of the rain-drops, owing to the varying velocity of the wind, which I assert to be the cause of the deficiency.

19. I will now approach the subject from an opposite point of view, and show *à priori* that the real increase of rain between the upper and lower gauges is not possible to any appreciable extent, according to the only physical explanation of the phenomenon which has ever been proposed. This theory was first suggested by Benjamin Franklin†, who compared a drop of rain to a bottle of cold water condensing dew upon itself when brought into a warm room. That rain, even in our hottest days, he adds, comes from a very cold region, is obvious from its falling sometimes in the form of ice.

This explanation has been repeated and adopted by almost all who have expressed any belief in the phenomenon. But others have shown its utter inadequacy; and the single calculated example given by Sir J. Herschel, in his recent excellent 'Essay on Meteorology‡,' may be adopted in our further discussions. "Admitting," he says, "a given weight of rain to arrive at 213 feet from the ground, with the temperature of the region at which it was formed unaltered, and supposing it to acquire in the remaining 213 feet the full temperature of the air (both of them extreme and, indeed, extravagant suppositions), admitting, too (though hardly less extravagant), the mean height of formation of the rain to be 12,000 feet, it would bring down with it a cold of 40° Fahr., which would condense (whether on the drops or in saturated air if diffused through it) only $\frac{1}{40}$ ths, or $\frac{1}{24}$th

* Annals of Philosophy, vol. xiv. p. 114.

† See his letter to Dr. Thomas Percivall, dated London 1771, in the 'Memoirs of Thomas Percivall, M.D.,' Appendix B.

‡ Page 104, as reprinted from the *Encyclopædia Britannica*, 8th ed.

$=0{\cdot}042$ of its weight, $=\frac{1}{17}$th of the quantity to be accounted for."

20. But, in reality, Sir J. Herschel's suppositions are far too favourable for the opinion which he opposes. In the first place, he makes no allowance for the heat derived from the gaseous air in addition to that received from the condensation of vapour. To estimate the amount of this, we may fairly make the assumption that has been found to give very exact results in the theory of the dry- and wet-bulb hygrometer. We may assume that the indefinitely thin film of air surrounding the drop of rain always takes the temperature of the drop, and yields up to it both the excess of its own sensible heat and the latent heat of the condensed aqueous vapour (the sensible heat of the aqueous vapour may be neglected as very minute). Then according to the formulæ of M. Auguste,—

Let w = weight of a volume of air equal to that of the film at 0° (Cent.).
α = coefficient of dilatation of a gas per degree of temperature.
t = temperature of the air.
t' = temperature of the drop.
f = elastic force of aqueous vapour at temperature t, the air being supposed saturated.
f' = elastic force of aqueous vapour at temperature t'.
h = height of the barometer.
δ = specific gravity of aqueous vapour.
γ = specific heat of dry air.
λ = latent heat of aqueous vapour.

Then

$$w \cdot \frac{1}{1+\alpha \cdot t'} \cdot \frac{h-f'}{760} \cdot (t-t') \cdot \gamma$$

will nearly represent the sensible heat given out by the film of air in cooling from t to t', and

$$w \cdot \delta \cdot \frac{1}{1+\alpha t'} \cdot \frac{f-f'}{760} \cdot \lambda$$

will be the amount of latent heat given out by the vapour condensed. The ratio of these is

$$\frac{1}{\delta} \cdot \frac{h-f'}{f-f'} (t-t') \frac{\gamma}{\lambda},$$

which varies with the value of t'. In Sir J. Herschel's example, let us suppose the lowest 213 feet of air to have the temperature of 60° F. Then t' is at first 20° F., and the above formula (inserting for δ its value ·6235; for γ, ·2669; for λ, $640-t'$; for h, ·760; and for f and f' their values from the common tables of

elastic force of aqueous vapour) has the value 1·072; that is to say, rather more heat is at first received from the cooling of the air than from the condensation of vapour. When the drop has increased in temperature to 40° F. ($t'=4°{\cdot}44$ C.), the value will be ·822, or the condensed vapour yields the larger share of heat; but even when the drop has the temperature 59°, the value has only diminished to ·624. Taking an average of these three determinations, we shall find that not more than 55 per cent. of the heat received by the drop will proceed from condensed vapour; consequently we must reduce Sir J. Herschel's first estimate almost to its half.

21. Again, considering that the temperature of the air increases uniformly from the elevation of 12,000 feet, at which Sir J. Herschel supposes the drop to be formed, down to its temperature at the surface, it is truly extravagant to suppose that a rain-drop should fall unaltered through 11,800 or 11,900 feet and then suddenly assume the full temperature of the air in the last 100 or 200 feet. A small drop falling very slowly will take the temperature of the air, or more strictly the temperature of evaporation, all the way down, and its degree of coldness on reaching the lowest stratum of air will be so slight as to produce no appreciable condensation even in perfectly moist air. On the other hand, a large drop falling so rapidly that it has no time to receive heat from the air, will indeed remain of a low temperature, but it will likewise have no time to receive heat from the lowest air. And drops of intermediate size, just in proportion as they fall more quickly and receive less heat from the upper strata of air, will be less able to receive heat from the lowest stratum.

22. Nor can it be argued that the rain-drop receives heat most freely in the lowest stratum of air because it there meets most vapour. For the humidity of the air invariably increases from the surface of the earth up to the first cloud, as was observed by Mr. Welch in each of his four balloon ascents. Even under the most rare or impossible hygrometric conditions the amount of condensation would be quite inappreciable. Under any usual or real conditions, it may be most confidently asserted that a falling drop of rain will either increase uniformly throughout its descent by an extremely minute quantity, or will, as is far more likely, evaporate and decrease by a small quantity. *Under no possible conditions will the increase within the last few hundred feet of descent be more than almost infinitesimal.*

23. It is of course perfectly well known and allowed that the temperature of rain is often much lower that that of the air at the surface. I have myself several times observed remarkably cold rain. So M. Boisgiraud* writes to the Paris Academy,

* *Annales de Chimie et de Physique* (sér. 2) vol. xxxiii. p. 417.

that by experiment he has proved rain to be sufficiently cold to produce precipitation even when the air is far from being saturated.

In the Greenwich Meteorological Observations for 1843 (p. 123) it is stated that, in occasional observations on the temperature of the rain, "It has been always found that when the rain has been warm with respect to the temperature of the air at the time, no differences have existed in the quantities of rain collected at the different heights; but that when the temperature of the air has been higher than the temperature of the rain, a difference has always existed." It is quite surprising that these writers do not perceive that their experiments tell directly against their own conclusions, or at least tell nothing at all to the purpose. If it is the rain in the lower gauge, as is most likely, which was found to be cold, it simply proves that condensation of vapour *has not taken place*, otherwise the rain would have been warmed thereby. If, however, the rain in the higher gauge be found of a low temperature, it tells us nothing at all to the purpose, unless we likewise prove that the same rain, on reaching the lowest gauge, is of a much higher temperature. In short, we must have a change of temperature observed; and such an observation has never been recorded, so far as I am aware.

24. As a further objection to the condensation theory, it may be added that Arago, in stating it*, argues that the difference of the rain collected in the two gauges should be greater as the air is more moist, a consequence which he confesses is not at all conformable to experience. This remark is strikingly borne out by the fact already stated, that the apparent increase of rain between the higher and lower gauges is usually greatest during the month of March. Now this is the month of prevalent dry, cold, north-east winds and gales, the very circumstances under which the condensation theory is most utterly inadequate or inapplicable.

25. A single secondary argument in favour of the supposed increase of rain-drops remains to be disposed of. Arago has remarked† that the internal supernumerary fringes of a rainbow are never seen on the lower parts of the bow near the surface of the earth. Now the supernumerary bows were explained by Dr. Young on the theory of interference of undulations; and their appearance indicates that the drops of rain upon which the bows appear are of exactly uniform size. Not observing the

* *Annuaire du Bureau des Longitudes*, pour l'an 1824, p. 161.

† "Il faut donc que pendant leur descente verticale, les gouttes d'eau aient perdu les propriétés dont elles jouissaient d'abord; il faut qu'elles soient sorties des conditions d'interférence *efficaces*; it faut qu'elles aient beaucoup grossi."

supernumerary bows in the lower part, but only in the upper, Arago argues that the condition of efficacious interference of the drops must have been destroyed in descending into the lower part of the atmosphere. "Therefore," he concludes, "the drops of rain must have much increased in size*." Obviously this does not in the least follow; for the condition of efficacious interference is uniformity of size†; and uniform drops, condensing moisture upon themselves, or evaporating in the same circumstances, will remain uniform in size. The disappearance of the supernumerary bows near the surface no doubt arises from the more disturbed current of air there causing the drops to encounter each other and coalesce irregularly, so that some drops are produced two or three times as large as the others.

26. Distant showers of rain are often seen distinctly to evaporate, and sometimes entirely vanish during their fall; but I have never observed or heard of a shower being observed to increase in density visibly during its descent.

27. It is now only right to add that both Arago‡ and Prof. Phillips have recorded unequivocally that a deficiency of rain in the upper gauge occurs even during a perfect calm. We have already quoted one such observation by Prof. Phillips§; and two others are found in his second paper on this subject‖. Prof. Phillips, indeed, considers that falling rain itself produces a downward current of air, which, it is just conceivable, might, by flowing over the sides of the upper rain-gauge or its support, deflect the rain. Again, while a perfect calm prevails on the ground, a gentle wind is usually blowing at the top of a lofty tower. As my explanation of the deficiency of rain in an elevated gauge is certainly inapplicable in a calm, I confess that my hearers must choose for themselves between considering two distinguished scientific observers capable of mistake in the observation of wind and calm on the one hand, and overturning some of the best established facts of physical science on the other hand.

28. If the present explanation be accepted, all observations by rain-gauges elevated or exposed to wind must be rejected as fallacious and worse than useless. But it is improbable that the error in a gauge with its mouth not more than one or two feet above the ground is worth considering. Still I believe that during a heavy shower almost all gauges lose a little rain by splashing, and it is worthy of consideration whether more accu-

* *Annuaire du Bureau des Longitudes*, pour l'an 1836, p. 300.

† Herschel's 'Meteorology,' p. 219. It seems likely, however, that Arago argued upon some other view of the cause of this phenomenon, which has been much misunderstood.

‡ *Annuaire du Bureau des Longitudes*, pour l'an 1824, p. 160.

§ See above, paragraph 13.

‖ Report of the British Association, 1834, Trans. Sections, p. 561.

rate means of estimation should not be adopted in regular observatories. The most unexceptionable rain-gauge would consist of a sheet of metal, many feet square (for instance 10 feet), spread flat upon the ground in an open place, with a flat collecting vessel in the centre connected by a pipe with a sunken reservoir or recording apparatus. The edges of the collecting vessel should not be higher than an inch, so as to present no appreciable obstacle or hollow space to the wind. At the same time nothing would be lost by splashing, as the splashes within and without the vessel would be equal.

29. My conclusions, shortly stated, are:—

(1) An increase of the rainfall close to the earth's surface is incompatible with physical facts and laws.

(2) The individual observations on this subject are utterly discordant and devoid of law when separately examined, and the process of taking an average under such circumstances gives an apparent uniformity which is entirely fallacious.

(3) When daily measurements of rain, or even monthly totals, are examined with reference to the strength of the wind at the time, it becomes obvious that there is a connexion.

(4) Wind must move with increased velocity in passing over an obstacle. It follows demonstratively that rain-drops falling through such wind upon the windward part of the obstacle will be further apart, in horizontal distance, than where the wind is undisturbed and of ordinary velocity.

London, August 28, 1861.

LV. *On the Cubic Centres of a Line with respect to Three Lines and a Line.*—Seond Note. *By* A. Cayley, *Esq.**

ON referring to my Note on this subject (Phil. Mag. vol. xx. pp. 418–423, 1860), it will be seen that the cubic centres of the line

$$\lambda x+\mu y+\nu z=0$$

in relation to the lines $x=0, y=0, z=0$, and the line $x+y+z=0$, are determined by the equations

$$x:y:z=\frac{1}{\theta+\lambda}:\frac{1}{\theta+\mu}:\frac{1}{\theta+\nu},$$

where θ is a root of the cubic equation

$$\frac{1}{\theta+\lambda}+\frac{1}{\theta+\mu}+\frac{1}{\theta+\nu}-\frac{2}{\theta}=0;$$

or as it may also be written,

$$\theta^3-\theta(\mu\nu+\nu\lambda+\lambda\mu)-2\lambda\mu\nu=0.$$

* Communicated by the Author.

Two of the centres will coincide if the equation for θ has equal roots; and this will be the case if

$$\lambda^{-\frac{1}{3}}+\mu^{-\frac{1}{3}}+\nu^{-\frac{1}{3}}=0,$$

or, what is the same thing, if $\lambda, \mu, \nu = a^{-3}, b^{-3}, c^{-3}$, where $a+b+c=0$. In fact, if $a+b+c=0$, then $a^3+b^3+c^3=3abc$, and the equation in θ becomes

$$\theta^3-\frac{3\theta}{a^2b^2c^2}-\frac{2}{a^3b^3c^3}=0;$$

that is,

$$(abc\theta)^3-3(abc\theta)-2=0,$$

which is

$$(abc\theta+1)^2(abc\theta-2)=0.$$

So that the values of θ are $\frac{-1}{abc}, \frac{2}{abc}$. First, if $\theta=-\frac{1}{abc}$, then x, y, z will be the coordinates of the double centre. And we have

$$\theta+\lambda=\frac{1}{a^3}-\frac{1}{abc}=\frac{1}{2a^3bc}(2bc-2a^2)$$

$$=\frac{1}{2a^3bc}(-a^2-b^2-c^2);$$

or putting for shortness $\square=a^2+b^2+c^2$,

$$\theta+\lambda=-\frac{1}{2a^3bc}\square, \quad =-\frac{3}{abc}\cdot\frac{\square}{6a^2},$$

with similar values for $\theta+\mu, \theta+\nu$. But $\frac{1}{x}, \frac{1}{y}, \frac{1}{z}$ are proportional to $\theta+\lambda, \theta+\mu, \theta+\nu$; and we may therefore write

$$\frac{\mathrm{P}}{x}=\frac{\square}{6a^2}, \quad \frac{\mathrm{P}}{y}=\frac{\square}{6b^2}, \quad \frac{\mathrm{P}}{z}=\frac{\square}{6c^2};$$

whence, in virtue of the equation $a+b+c=0$, we have for the locus of the double centre,

$$\sqrt{x}+\sqrt{y}+\sqrt{z}=0.$$

Or this locus is a conic touching the lines $x=0, y=0, z=0$ harmonically in respect to the line $x+y+z=0$, a result which was obtained somewhat differently in the paper above referred to.

Next, if $\theta=\frac{2}{abc}$, x, y, z will be the coordinates of the single centre. And we now have

$$\theta+\lambda=\frac{1}{a^3}+\frac{2}{abc}=\frac{1}{2a^3bc}(2bc-2a^2+6a^2)=\frac{1}{2a^3bc}(-\square+6a^2)$$

$$=-\frac{3}{abc}\frac{\square-6a^2}{6a^2},$$

with similar values for $\theta+\mu$, $\theta+\nu$. But $\frac{1}{x}, \frac{1}{y}, \frac{1}{z}$ are proportional to $\theta+\lambda$, $\theta+\mu$, $\theta+\nu$, and we may therefore write

$$\frac{\mathrm{P}}{x}=\frac{\square-6a^2}{6a^2},\quad \frac{\mathrm{P}}{y}=\frac{\square-6b^2}{6b^2},\quad \frac{\mathrm{P}}{z}=\frac{\square-6c^2}{6c^2},$$

from which equations, and the equation $a+b+c=0$, the quantities P, a, b, c have to be eliminated. I at first effected the elimination as follows: viz., writing the equations under the form

$$\frac{x}{x+\mathrm{P}}=\frac{6a^2}{\square},\quad \frac{y}{y+\mathrm{P}}=\frac{6b^2}{\square},\quad \frac{z}{z+\mathrm{P}}=\frac{6c^2}{\square},$$

we obtain

$$\frac{x}{x+\mathrm{P}}+\frac{y}{y+\mathrm{P}}+\frac{z}{z+\mathrm{P}}=6,$$

$$\sqrt{\frac{x}{x+\mathrm{P}}}+\sqrt{\frac{y}{y+\mathrm{P}}}+\sqrt{\frac{z}{z+\mathrm{P}}}=0,$$

which are easily transformed into

$$\frac{x}{x+\mathrm{P}}+\frac{y}{y+\mathrm{P}}+\frac{z}{z+\mathrm{P}}=6,$$

$$\frac{yz}{(y+\mathrm{P})(z+\mathrm{P})}+\frac{zx}{(z+\mathrm{P})(x+\mathrm{P})}+\frac{xy}{(x+\mathrm{P})(y+\mathrm{P})}=9;$$

or, what is the same thing,

$$6(\mathrm{P}+x)(\mathrm{P}+y)(\mathrm{P}+z)-x(\mathrm{P}+y)(\mathrm{P}+z)-y(\mathrm{P}+z)(\mathrm{P}+x)$$
$$-z(\mathrm{P}+x)(\mathrm{P}+y)=0,$$
$$9(\mathrm{P}+x)(\mathrm{P}+y)(\mathrm{P}+z)-yz(\mathrm{P}+x)\qquad -zx(\mathrm{P}+y)$$
$$-xy(\mathrm{P}+z)=0,$$

which give

$$6\mathrm{P}^3+5\mathrm{P}^2(x+y+z)+4\mathrm{P}(x+y+z)+3xyz=0,$$
$$9\mathrm{P}^3+9\mathrm{P}^2(x+y+z)+8\mathrm{P}(x+y+z)+6xyz=0.$$

Or, multiplying the first equation by 2, and subtracting the second,

$$3\mathrm{P}+x+y+z=0;$$

and we thus obtain for the locus of the single centre the equation

$$\frac{x}{-2x+y+z}+\frac{y}{-2y+z+x}+\frac{z}{-2z+x+y}=2,$$

or, what is the same thing,

$$x^3+y^3+z^3-(yz^2+zx^2+xy^2+y^2z+z^2x+x^2y)+3xyz=0,$$

which may also be written,

$$-(-x+y+z)(x-y+z)(x+y-z)+xyz=0.$$

The same result may also be obtained as follows: viz., observing that $\square - 6a^2 = b^2 + c^2 - 5a^2 = -4a^2 - 2bc$, we have

$$\frac{x}{\mathrm{P}} = \frac{-3a^2}{2a^2+bc}, \quad \frac{y}{\mathrm{P}} = \frac{-3b^2}{2b^2+ca}, \quad \frac{z}{\mathrm{P}} = \frac{-3c^2}{2c^2+ab};$$

and then by means of the equation

$$\frac{a^2}{2a^2+bc} + \frac{b^2}{2b^2+ac} + \frac{c^2}{2c^2+ab} - 1 = 0,$$

which is identically true in virtue of $a+b+c=0$ (in fact, multiplying out, this gives

$$12a^2b^2c^2 + 4(b^3c^3+c^3a^3+a^3b^3) + abc(a^3+b^3+c^3)$$
$$-8a^2b^2c^2 - 4(b^3c^3+c^3a^3+a^3b^3) - 2abc(a^3+b^3+c^3) - a^2b^2c^2 = 0;$$

that is,

$$3a^2b^2c^2 - abc(a^3+b^3+c^3) = 0, \text{ or } abc(a^3+b^3+c^3-3abc) = 0,$$

where the second factor divides by $a+b+c$), we find the above-mentioned equation,

$$x+y+z+3\mathrm{P}=0.$$

We then have

$$\frac{-x+y+z}{\mathrm{P}} = \frac{x+y+z}{\mathrm{P}} - \frac{2x}{\mathrm{P}} = -3 + \frac{6a^2}{2a^2+bc} = -\frac{3bc}{2a^2+bc};$$

that is,

$$\frac{-x+y+z}{\mathrm{P}} = \frac{-3bc}{2a^2+bc}, \quad \frac{x-y+z}{\mathrm{P}} = \frac{-3ca}{2b^2+c}, \quad \frac{x+y-z}{\mathrm{P}} = \frac{-3ab}{2c^2+ab}.$$

And forming the product of these functions, and that of the foregoing values of $\frac{x}{\mathrm{P}}, \frac{y}{\mathrm{P}}, \frac{z}{\mathrm{P}}$, we find as before,

$$-(-x+y+z)(x-y+z)(x+y-z)+xyz=0$$

for the equation of the locus of the single centre. The equation shows that the locus is a cubic curve which touches the lines $x=0$, $y=0$, $z=0$ at the points where these lines are intersected by the lines $y-z=0$, $z-x=0$, $x-y=0$ (that is, it touches the lines $x=0$, $y=0$, $z=0$ harmonically in respect to the line $x+y+z=0$), and besides meets the same lines $x=0$, $y=0$, $z=0$ at the points in which they are respectively met by the line $x+y+z=0$.

2 Stone Buildings, W.C.
September 25, 1861.

LVI. *On Earth-currents, and their Connexion with the Phenomena of Terrestrial Magnetism. By the* Rev. H. Lloyd, *D.D., D.C.L.**

IN the year 1848 Mr. Barlow communicated to the Royal Society a paper "On the Spontaneous Electrical Currents observed in the Wires of the Electric Telegraph," in which he established the important fact, that a wire, whose extremities are connected with the earth at two distant points, is unceasingly traversed by electric currents, the intensity of which varies with the azimuth of the line joining the points of contact with the ground. The direction of these currents was proved to be the same at both extremities of the same wire, and was shown to depend on the relative positions of the earth-connexions, while it was wholly independent of the course followed by the wire itself. The currents cease altogether when either of the contacts with the earth is interrupted. From these facts Mr. Barlow concluded that "the currents are terrestrial, of which a portion is conveyed along the wire, and rendered visible by the multiplying action of the coil of the magnetometer."

Mr. Barlow further observed that, apart from sudden and occasional changes, the general direction of the needle of the galvanometer appeared to exhibit some regularity. He was thus led to institute a series of observations for fourteen days and nights, on two wires simultaneously, one from Derby to Rugby, and the other from Derby to Birmingham, the positions of the needles in both circuits being recorded every five minutes, day and night. From these observations he concluded—

"1. That the path described by the needle consisted of a regular diurnal motion, subject to disturbances of greater or less magnitude.

"2. That this motion is due to electric currents passing from the northern to the southern extremities of the telegraph wires, and returning in the opposite direction.

"3. That, exclusive of the irregular disturbances, the currents flowed in a southerly direction from about 8 or 9 A.M. until the evening, and in a northerly direction during the remainder of the twenty-four hours."

He was thus led to examine whether any relation subsisted between these movements and the daily changes of the horizontal magnetic needle. And having made for this purpose a series of simultaneous observations with a delicate declinometer, he came to the conclusion, that although generally the currents flow *southwards* during that part of the day in which the variation of

* Communicated by the Author, having been read at a meeting of the Royal Irish Academy, held November 11, 1861.

the horizontal needle is *westerly* (*i. e.* from 8 or 9 A.M. until the evening), and *northwards* when the variation is *easterly* (*i. e.* during the night and early part of the morning), "yet simultaneous observations showed no similarity in the path described by the magnetic needle and the galvanometer."

An examination of Mr. Barlow's galvanometric observations led me, some time since, to an opposite conclusion; and at the last meeting of the British Association I stated my conviction, founded on these observations, that the earth-currents, whose continuous flow Mr. Barlow has the merit of establishing, would eventually explain all the changes of terrestrial magnetism, both periodic and irregular. I now proceed to state the grounds of this conviction, and to show, from Mr. Barlow's observations, that the diurnal changes of the earth-currents correspond with those of the horizontal component of the earth's magnetic force*.

Let us suppose, then, that the forces which act upon the horizontal needle, and which cause it to deviate from its mean position, are due to electric currents traversing the upper strata of the earth in a horizontal direction; and let ξ denote the intensity of the current in the magnetic meridian, *positive* when flowing *northwards*, and *vice versâ*; and η the intensity of the current perpendicular to the magnetic meridian, *positive* when flowing *eastward*, and *vice versâ*. Then the force of the current in any direction, making the angle ϵ with the magnetic meridian (measured to the east of north), is

$$\phi = \xi \cos \epsilon + \eta \sin \epsilon.$$

Now ξ is proportional to the force which deflects the freely suspended horizontal needle from its mean position, or to $X\Delta\psi$, X being the horizontal component of the earth's magnetic force, and $\Delta\psi$ the change of declination expressed in parts of radius. Similarly, η is proportional to the force which deflects from its mean position a magnet which is maintained (by torsion or

* The first proof of a correspondence between the magnetic variations, and the changes of the earth-currents, seems to be due to Dr. Lamont of Munich, in a letter dated July 29, 1861, which was read by the Astronomer Royal at the last meeting of the British Association. Dr. Lamont states that he has found "that electric currents, or (as they may be more properly termed) *electric waves*, varying in direction or intensity, are constantly passing at the surface of the earth, and that these waves correspond perfectly with the variations of terrestrial magnetism." The correspondence here referred to seems to relate to the *smaller* and more *rapid variations* of the terrestrial magnetic force. But in a letter to Prof. Heiss, dated September 1, Dr. Lamont expresses his conviction that the whole diurnal movements are due to these earth-currents. He adds, however, that he had hitherto been unable fully to verify this conclusion, owing to the continual changes produced in the collecting plates and in the wires by heat and moisture.

other means) in a position perpendicular to the magnetic meridian, and is measured, in terms of X, by the relative changes of the horizontal intensity taken negatively. Hence the force of the current in any given direction may be determined in terms of the same units.

Now $$\epsilon = \alpha - \psi,$$

in which α is the azimuth of the line connecting the two stations measured from the true meridian eastward, and ψ the magnetic declination measured in the same direction. The observations of Sir James Ross at Derby, give $\psi = -22^\circ\ 25'$; and we have, for the line connecting Derby with Rugby,

$$\alpha = -13^\circ\ 7', \quad \alpha - \psi = +9^\circ\ 18';$$

and for the line joining Derby and Birmingham,

$$\alpha = +33^\circ\ 27', \quad \alpha - \psi = +55^\circ\ 52'.$$

The first column of the following Table contains the mean variations of the magnetic declination at the alternate hours for the month of May, as deduced from four years' observation of that element at the Dublin Magnetic Observatory; the second contains the corresponding values of the changes of the horizontal intensity, in ten thousandths of the whole intensity; and the third and fourth the calculated values of the deflecting forces in the line perpendicular to that connecting the earth-contacts at Derby and Rugby and at Derby and Birmingham respectively, and expressed in terms of the same units. These latter numbers are by hypothesis proportional to the intensities of the currents directed along the connecting wires.

TABLE I.—*Calculated* Values of the Intensity of the Currents traversing the Wires uniting Derby and Rugby, and Derby and Birmingham, respectively.

Hour.	$\Delta\psi$.	$\frac{\Delta X}{X}$.	Derby and Rugby.	Derby and Birmingham.
1 A.M.	1·8	0·4	5·1	2·6
3	2·5	− 1·6	7·6	5·5
5	3·9	− 3·7	11·9	9·5
7	5·2	− 8·4	16·2	15·4
9	2·1	−16·9	8·9	17·5
11	−4·1	−15·9	− 9·3	6·4
1 P.M.	−7·1	− 3·1	−19·8	− 9·0
3	−5·1	6·1	−15·7	−13·4
5	−1·8	14·2	− 7·6	−14·8
7	0·3	14·6	− 1·5	−11·6
9	1·0	9·0	1·3	− 5·9
11	1·3	5·2	2·9	− 2·2

The galvanometric observations instituted by Mr. Barlow on

these two lines were continued for fourteen consecutive days, commencing May 17, 1848. Of these days of observation, however, six are incomplete, viz. May 17, 19, 20, 23, 24, 30; and another day (May 27) appears from the Dublin observations to have been a day of considerable magnetic disturbance. Omitting these, as unsuited to furnish true mean results, the means of the remaining days are as follow. The *positive* numbers indicate currents proceeding *towards* Derby, and the *negative* currents in the contrary direction.

TABLE II.—Mean *observed* Values of the Intensity of the Currents traversing the Wires uniting Derby and Rugby, and Derby and Birmingham, respectively.

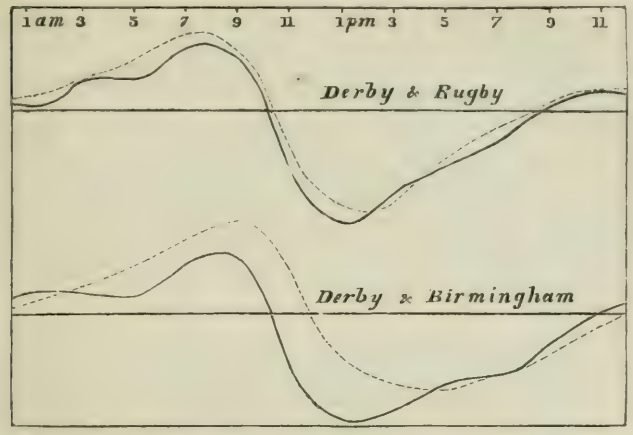
Hours.	Derby and Rugby.				Derby and Birmingham.			
	A.M.		P.M.		A.M.		P.M.	
1	−1·4	0·3	−5·0	−5·1	0·2	1·5	−9·1	−8·5
2	2·5		−5·5		2·9		−7·7	
3	1·6	1·7	−2·7	−3·3	0·9	1·3	−7·4	−7·4
4	1·1		−2·4		0·7		−7·2	
5	0·5	1·2	−1·8	−2·3	0·6	1·2	−3·6	−5·1
6	2·7		−3·2		2·8		−6·3	
7	3·1	3·0	−0·6	−1·1	3·9	4·1	−4·5	−4·7
8	3·1		−0·2		5·9		−3·4	
9	2·4	1·8	0·4	0·2	4·2	3·4	−0·8	−1·7
10	−0·9		0·1		−0·6		−1·7	
11	−4·3	−3·6	0·4	0·6	−7·2	−5·8	0·3	0·4
12	−5·1		1·7		−8·1		2·8	

It will be observed that the changes indicated by these numbers are very systematic. In the wire connecting Derby and Birmingham the current flows *southwards* from 10 A.M. to 10 P.M. inclusive, and *northwards* during the remaining hours. In the wire connecting Derby and Rugby the *southward* current lasts from 10 A.M. to 8 P.M. inclusive, and it is *northward* (with a single exception) during the remaining hours. There are, however, as might be expected in so short a series, some irregularities in the course of the changes. In order to lessen these, and at the same time to confine the results to such as are comparable with the preceding, I have given (in the alternate columns of the Table) the means corresponding to the alternate hours commencing at 1 A.M. computed by the formula

$$\tfrac{1}{4}(a+2b+c).$$

The numbers so obtained are projected into curves in the annexed diagram, having been previously multiplied by constant coefficients in order to equalize the ranges with those of the computed results. The dotted lines in both cases are the correspond-

ing projections of the calculated results. The agreement between these two sets of curves is probably as great as could be expected



in the results of so short a series of observations; and we seem therefore entitled to conclude that the diurnal movements of the two horizontal magnetometers are accounted for by electric currents traversing the upper strata of the earth.

There is one point of difference to which it is important to draw attention. It will be seen that the *calculated* curves are for the most part *above the observed*. The reason of this will be evident upon a little consideration. The *zero* from which the calculated results are measured is the mean of the day; whereas that of the observed results is the *true zero*, corresponding to the absence of all current. Now the chief deflections of the galvanometer needle (as appears from the latter curves) are those in which the sun is above the horizon; and the *zero line* consequently divides the area of the diurnal curve unequally, being considerably nearer to the night observations than to those of the day. If the calculated curves be *displaced* by a corresponding amount, their agreement with the observed will be much closer.

The difference here noted is one of considerable theoretical importance. Magnetometric observations furnish merely *differential* results, the magnitude and the sign of which have reference solely to an *arbitrary zero*. We are accordingly ignorant even of the relative values of the effects, and are unable to compare them with their physical causes, whether real or supposed. In these respects the galvanometric observations have the advantage. In them, *positive* and *negative* are physically distinguished by the *direction* of the currents; and this, as well as

the absence of all currents, is indicated by the instrument itself; the results therefore furnish the measures of the forces by which they are produced.

The next and most important step in this inquiry will be to assign the physical cause of these phenomena. The existence of electric currents traversing the earth's crust has hitherto been maintained as a *hypothesis*, on account of its supposed adequacy to explain the terrestrial magnetic changes. Now, however, their existence is proved not only to be a *fact*, but also a fact sufficient to explain the phenomena. It remains therefore only to ascertain their source; and it will be for those who deny that the sun operates by its *heat* in producing the phenomena of terrestrial magnetism, to assign to these currents a more probable origin.

P.S. While these pages were passing through the press, the writer received, by the kindness of Dr. Lamont, a copy of a further communication from him on the same subject, in a letter to Professor De la Rive, dated Oct. 10, 1861. In this letter, Dr. Lamont seems to recede from the view expressed in a former letter (see note, *suprà*), and expresses his belief that the diurnal variations of terrestrial magnetism cannot be explained by the direct action of electric currents propagated on the earth's surface; and he advances the hypothesis, that the regular portion of these variations is due to a peculiar influence of the sun, their irregular fluctuations alone being caused by the earth-currents. These conclusions seem to be irreconcilable with Mr. Barlow's observations, and are opposed to the inferences which I have drawn from them in the preceding pages.

The scientific public will therefore await with interest the detailed publication of Dr. Lamont's investigations on this important subject, in which, it may be hoped, he will give some clue to the explanation of this seeming discordance.

Trinity College, Dublin,
Nov. 16, 1861.

LVII. *Considerations respecting the Original Formation of Aërolites.*—Part II. *By* W. Haidinger, *For. Mem. R.S.L. & E. and Director-General of the Geological Survey of Austria.*

[Concluded from p. 361.]

IF the phenomena attending the fall of meteorites upon our own earth offer serious difficulties, considerations concerning the condition of their previous existence is by far a more

arduous task. It must not be forgotten that there are two cosmical or planetary bodies in question; the one a large one (our own globe), and a comparatively minute one (the meteorite). M. Leverrier, to whose talents and genius as an astronomer and mathematician we chiefly owe the discovery of the planet Neptune, felt himself authorized to pronounce, before the Paris Academy (October 1, 1860), a view or suspicion which he himself designates as "strange at the first aspect, but very possibly a reality*," viz. that in comparatively recent times new and small planets have been formed out of planetary matter existing at different distances around the sun, and possessing various degrees of density and volume†, but that their existence had remained unperceived till, during the last few years, the extraordinary amount of attention bestowed on the subject had at length been rewarded by a number of discoveries ‡.

The original formation and constitution of cosmical bodies have of late become the subject of the most diversified consideration. Some have tried to develope peculiarities previously more or less neglected; others (as my respected friend Prof. C. F. Naumann, in his classical 'Manual of Geology,' chapter on the Temperature of the Interior of the Globe, 2nd edit. 1857, vol. i. p. 36) have endeavoured to treat the question in a lucid and exhaustive synopsis, and to collect into a whole the opinions of men of the

* "Une idée, un soupçon, étrange peut-être au premier abord, mais qui peut très-bien être une réalité."—Moigno's *Cosmos*, 1860, vol. ix. p. 476.

† "L'espace autour du soleil est, on le sait, rempli de matière cosmique, et de matière cosmique de tous degrés de ténuité et de grosseur."—Ibid.

‡ As closely related to this portion of M. Haidinger's paper, the following extract from the 'Annual Register of Facts and Occurrences' for August 1861, may be here appropriately inserted:—"M. Leverrier, from the perturbations observed in the orbits of the planets Mercury, Venus, the Earth, and Mars, has still more recently come to the conclusion that there exists in our own system a considerable quantity of matter which has not hitherto been taken into account. In the *first* place, he supposes that there must exist within the orbit of Mercury, at about 0·17 of the Earth's distance from the Sun, a mass of matter nearly equal in weight to Mercury. As this mass of matter would probably have been observed before this, either in transit over the Sun's disc, or during total eclipses of the Sun, if it existed as one large planet, M. Leverrier supposes that it exists as a series of asteroids. *Secondly*, M. Leverrier sees reason to believe that there must be a mass of matter, equal to about one-tenth of the mass of the Earth, revolving around the Sun at very nearly the same distance as the Earth. This also he supposes split up into an immense number of asteroids [? meteorites]. *Thirdly*, M. Leverrier's researches have led him to the conclusion that the group of asteroids which revolve between Mars and Jupiter, sixty of which have already been seen and named, and had their elements determined, must have an aggregate mass equal to one-third of that of the Earth. He likewise thinks it is not unlikely that similar groups of asteroids exist between Jupiter and Saturn, Saturn and Uranus, and between Uranus and Neptune." See also *Cosmos* for June 1861, p. 639.—R. P. G.

highest authority, rather for the purpose of respectful study, than to be made the subject of control or contradiction. Proceeding from simple correlations, I humbly venture to enunciate some few considerations respecting the formation of meteorites, which, eminently diversified as they are if taken individually, I must yet consider, along with Sir David Brewster, Prof. Laurence Smith, and other naturalists, to be fragments of a larger or more voluminous body.

The formation of crystals requires a movement of molecules. This is a general and most irrefragable theorem. We see crystals deposited from gaseous and liquid solutions, or wherever the single molecules have acquired mobility under the influence of high temperature, as in substances in a state of fusion.

Whenever solid bodies are undergoing metamorphic changes, crystals form out of pulverulent, as well as out of relatively solid substances, when they undergo influences that make their intimate particles moveable. We do not know that crystallization can take place under any other circumstances, so long as the laws of nature, as now known to us, remain in force. We are entitled therefore to conclude that these bodies, coming from cosmical space into our atmosphere, took their point of departure from matter either in a gaseous, liquid, or pulverulent condition. The real point of departure then is matter in the form of an *impalpable powder*, assumed to be the initial deposit of any substance suspended in a gaseous or liquid solution.

Meteoric *stones*, almost pulverulent in their nature, with opake, nearly earthy fracture (as those of Reichenbach's second family), others whitish, without rounded particles, or dark-coloured (as those of Bokkeveld), are connected, by a long series of intermediate forms, with the highly crystalline meteorites of Chassigny, Juvenas, Shalka, and the solid compact ones of Seres, Tabor, Chantonnay, Segowlee, Parnallee, &c. In the same way a long series of structural transitions connect the non-crystalline meteoric *irons* of the Cape of Good Hope and Hemalga with the beautifully crystalline varieties of Agram, Elbogen, Lenarto, Lockport, Red River, Nebraska, ending with the most perfect type, that of Braunau. The crystals of olivine contained in the meteorites of Hainholz, Brahin, Atacama, and Krasnojarsk prove the power of crystallization to have remained active during a long period of time.

With our present knowledge of natural laws, these characteristically crystalline formations could not possibly have come into existence except under the action of *high temperature* combined with *powerful pressure*; though we have to search in vain for a *heated cosmical space*, as supposed by Poisson.

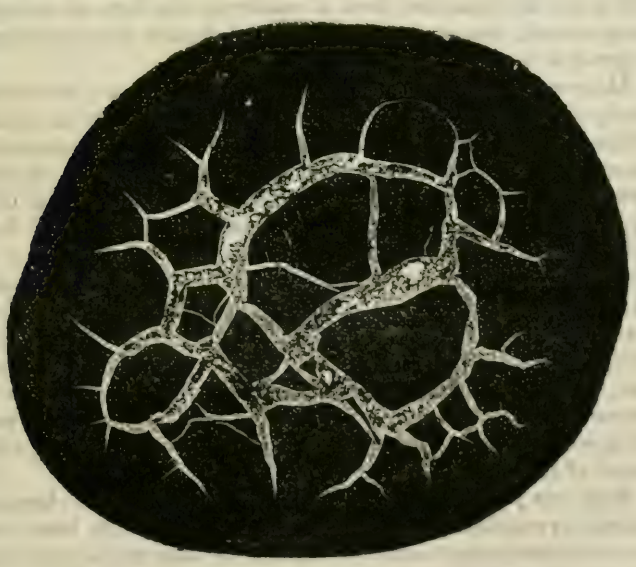
If we suppose within the glacial cold of space the existence of

a pulverulent aggregate of all the substances found in meteorites, these could not be brought to crystallize gradually without some means or source by which heat could subsequently act upon them; and it may be questionable how far the *mutual pressure of masses*, or the attraction of a great whole on its isolated and still unconnected particles, may possibly suffice to produce such an effect.

I may here anticipate that a mere pulverulent aggregate having a rotatory movement in space must necessarily also acquire a spheroidal form dependent upon rotation, exactly like a liquid (according to Professor Plateau's experiments) not acted on by terrestrial attraction, and consequently in a state of free suspension.

A *septaria*, an object familiar to mineralogists and geologists, may serve to convey an idea of the effects of pressure acting from the circumference to the centre. Septariæ are spheroidal tuberiform bodies, occasionally slightly compressed in one direction (see fig 3), consisting of an external solid shell or crust of compact argillaceous sphærosiderite, filled up with the same substance, and intersected by numerous and somewhat imperfect veins of calcareous and magnesio-calcareous spar. Fig 3 is an autotype, taken from a specimen in the Imperial Museum of

Fig. 3.

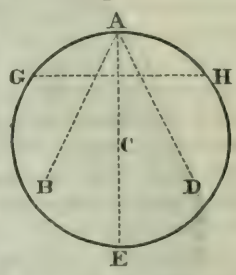


Vienna. The formation of such a septaria may be explained as follows:—within a stratum of clay, the particles richest in the carbonate of oxide of iron agglomerate or coalesce: the clay-

stratum, and with it the sphærosideritic agglomeration, undergoes pressure, which, if sufficient, leaves in the interior a softer portion, more impregnated with water than the external crust from which that element has been squeezed more completely out. The sphærosiderite is naturally inclined to assume throughout the consistence of the external crust, which, like a vault or arch, acts in every direction against further contraction. Contraction ensues, and the fissures produced in consequence are subsequently filled up with crystalline deposits of substances held in solution by liquids penetrating, or *already contained* within, the interstices. At first magnesian carbonate of lime, then calcareous spar (occasionally also iron pyrites) are separated and deposited. Certainly there seems to exist a great analogy between the process of formation of such septariæ and that admissible as going on within a large pulverulent globe freely suspended in space. There is indeed no external pressure, but every stratum of ponderable matter exercises compression on the whole.

The following figure (fig. 4) is taken from Professor C. Koppe's "Physik und Meteorologie" (in Bädeker's collective publication, *Die gesammten Naturwissenschaften*). A point A, attracted at the surface by a material point B as a sum of many others, undergoes also attraction from another point D in a similar situation. The resulting line of direction falls between B and D, and passes through the centre C, along the line C E.

Fig. 4.



No determinate direction could prevail in the centre itself, where the mass of the sphere is uniformly distributed, and there the action of gravitation would completely cease (or be in equilibrium). As each particle on the surface tends to sink towards the centre, it finds an obstacle from another immediately subjacent, this from a third, &c., and this obstacle must be overcome or removed. The particles, at first unconnected, join or approximate more and more slowly; *pressure* is beginning and increasing. As on the surface of our globe, so we may suppose to have existed in meteorites, combinations of heterogeneous elements very different from each other in their specific gravities. Among other substances found in meteorites are oxygen, sulphur, phosphorus, carbon, chromium, silicium, hydrogen, cobalt, nickel, iron, aluminium, magnesium, calcium, potassium,—all of them extremely discrepant in density and other physical qualities. It is doubtful whether these existed as elementary particles, or in chemical combinations. In

the present case, that first supposed may in reality have been precedent to the second mode or condition of existence.

Such a supposition may be considered more admissible than the views now more prevalent, that cosmical space possessed such an elevated temperature that the whole of matter existed in a gaseous state at the rate (as Vogt has calculated*) of only $\frac{13}{1,000,000}$ ths of a grain within the space of one cubic (German) mile. Such a supposition, however, lies far beyond us as regards experimental proof, even should we succeed, by connecting the past with the present, in producing correlations that might comparatively be considered "initial" ones. If the heavier metallic particles tend downwards along with others of less density which are pushed aside or even forced to ascend, while the whole surface pressing towards the centre is consequently continually diminishing in bulk, friction must unavoidably follow, and with it (as experience teaches) development of electricity and heat. We are, however, sufficiently acquainted with the phenomena attending the mutual combination of several among the above-named substances, as connected with combustion, oxidation, and chemical action in general, to enable us to pursue this part of our examination further.

On a former occasion (in "eine Leitform der Meteoriten," &c., Imp. Acad. Proceedings, vol. xl. p. 539) I mentioned an important communication from my respected colleague, Professor Schrötter, concerning the fact that substances whose mutual action under ordinary temperature goes on violently and with every appearance of intense combustion (as chlorine acting upon phosphorus, antimony, arsenic, or ammonia), when refrigerated to $-80°$ in a mixture of solid carbonic acid and ether (so that chlorine is liquified under ordinary barometric pressure), remain in a state of complete mutual indifference. Under these circumstances, a slight elevation of temperature, especially if care has not been taken to keep up a low temperature by rapid evaporation, may be the cause of dangerous explosions.

The same is the case with *alcohol* and chromic or chlorochromic acid, with *ammonia* and chloride of phosphorus, with iodine or bromine and phosphorus (see Professor Schrötter's *Die Chimie nach ihrem gegenwärtigen Zustande, &c.*, Vienna, 1847 vol. i. p. 129). Professor Dumas reported on this fact in the Paris Academy (*Comptes Rendus*, January 1845, No. 3, p. 193), remarking that he had not been able to observe a complete inactivity,—probably, as Professor Schrötter now objects, because, accelerated evaporation having not been duly provided for, the

* Nöggerath in "Geognosie und Geologie," in Bädeker's above-quoted 'Collective Publication.'

elevation of temperature in conducting the experiment took place too rapidly.

When chemical action has once commenced, a continuous increase of temperature easily takes place, till, beneath the uppermost dry and pulverulent surface still exposed to the intense cold of cosmical space, a crust or shell has been formed, within which the atoms of matter, following the influence of their own peculiar forces and properties, unite in chemical combinations, and individualize themselves into separate crystals whose elevated temperature (chemical action having ceased) effects more or less lithoid consistence.

The attempts to explain the central heat of the Earth by means of electrical and chemical action come near the views enounced by Sir Charles Lyell, Prof. De la Rive, &c. (see Naumann, *loc. cit.* p. 63), while the compressive action of the uppermost terrestrial strata, here taken for a point of departure, is quite adequate to the conditions required by an uninterrupted process of induction. The above-named mobility of particles once admitted, the frequent occurrence of globules in meteorites is no longer a matter of surprise. These globules, sometimes rather regularly rounded (as in some oolites), and in other cases angular or fragmentary (with edges occasionally rounded at the same time, however), are imbedded in an agglomeration of looser and frequently arenaceous particles, for which I have proposed the name of "meteoric tufa*." The surface of these globules is characteristically surrounded with particles of iron, as in the meteorites of Seres, Assam, Renazzo, Parnallee, and others. The meteorites presenting the aspect of crystalline rocks unmixed with native iron, as in those of Chassigny, Juvenas, Shalka, &c., stand far higher in the scale of development than even those most compact meteorites which include minute particles of metallic iron dispersed through an arenaceous, granular, or tufaceous aggregate of lithoid substance. The highest stage of development is exemplified by the pure and highly crystalline meteoric irons, partially resembling the contents of metalliferous veins (as in the Agram iron), and partly surrounded in all directions with smooth surfaces, a still unexplained circumstance even if superficial oxidation during their progress through the terrestrial atmosphere is taken into account. Instances of a vein-like disposition of metallic iron (as in the Macao meteoric stone), or of iron pyrites (in those of Pegu, Allahabad), as well as genuine planes of fissure (Stannern), rough (Allahabad), or specular (Ensisheim, Lixna, &c), exactly like those in our terrestrial rocks, are of no rare occurrence in many meteorites. The me-

* See Haidinger's paper, "Das von Herrn Dr. Auerbach entdeckte Meteoreisen von Tula," Imperial Academy, Meeting of November 29, 1860.

teoric iron of Tula, containing imbedded fragments of meteoric stone, discovered by Dr. Auerbach, proves beyond all doubt the occurrence of larger iron-masses in veins, and of their including fragments of the adjacent rocks*.

In his paper "On Meteorites in Meteorites" (Poggendorff's *Annalen*, 1860, vol. cxi. p. 353), Baron Reichenbach examines the mechanical composition of meteorites, paying particular attention to their rounded or angular particles, these last characterized as "fragments, broken and rolled pieces, and pebbles" (*loc. cit.* p. 384). Thirty-two meteorites (? stones), microscopically analysed, presented in their intimate or mechanical composition *five* distinct different substances, viz. sulphuret of iron (pyrites), native iron, oxidulated oxide of iron (magnetite), a grey, and a black substance†. Leaving aside some peculiarities in the terminology employed by Baron Reichenbach, as well as his criticism (p. 379) on the expression "secretion," stated to have been used by myself, while in fact I prefer the more neutral term "included substances," I could not give a better mode of considering in detail the structure of meteorites than has been rendered by Baron Reichenbach himself; and indeed the scientific world is obliged to him for it. There we have the character and nature of "meteoric tufa" pursued into their minutest details, indicating *successive* formation by the junction of the more intimate atoms of "cosmical dust,"—though, and this is the very foundation of either mode of consideration, this took place *not within the vaporous dust freely dispersed* through cosmical space, but within an *already* pre-existent and voluminous agglomeration, in which mutual attraction only became effective by producing real or absolute pressure. I really feel obliged to Baron Reichenbach for these statements, although undertaken with other intentions than to illustrate my own views on this matter.

The influence of solar heat has purposely been neglected in the preceding considerations, on account of the want of an atmosphere, in the strict sense of the term, in those spaces within which the formation of meteorites (in their *initial* condition or movement) may be admitted to take place. We know the temperature of planetary space to be far below that of the freezing-point, and we may assume an identical condition for the entire orbit of our globe, with a radius of 95,000,000 miles, as well as for the spaces beyond the orbit of Neptune (thirty times the distance of the Earth from the Sun); and even still further,

* See Haidinger's paper "On the Tula Meteoric Iron," *l. c.* note *ante*.

† These black and grey substances must refer to *stony particles*. I think Reichenbach's list might be extended so as to include magnetic pyrites (pyrrhotine), as well as a white substance.—R. P. G.

where probably more than one planet, and certainly comets, are pursuing their course, the solar distance of Neptune being itself only $\frac{1}{7000}$th of the interval between the Sun and the nearest fixed star*. During the period which the Earth takes to accomplish her annual revolution round the Sun, the latter, together with the whole solar system, has progressed (at the rate of about seven German miles a second) through a space of which the Earth's distance from the Sun is only the eleventh part†. Professor Koppe‡ says, "All circumstances agree in confirming the supposition that, for a period of 3300 years, the average temperature of Palestine has not undergone any notable change." During this period our globe has run in length some 36,300 times its own distance from the Sun—a course not to be achieved by light itself in less than 209 days, though this enormous distance is small indeed compared with the unlimited range of space itself!

Taking for granted that the weight of meteorites falling upon our earth's surface amounts yearly to 450,000 lbs. (Vienna weight), if not more§, and consequently to 450 millions of pounds in a millennial period, Baron Reichenbach has brought under consideration the question, whether in the course of ages such an increase of ponderable matter would not be without notable influence on other as well as physical correlations connected with our globe in the solar system‖. The length of such periods as are here taken into account is after all almost too enormous for our imagination to grasp: to accomplish the formation of a meteoric agglomeration equal in size to our globe would require 3000 trillions of years.

Another consideration, however, may here find appropriate notice. We may ask, if then our globe in the course of one solar revolution can thus admit of an increase of matter to the amount of 450,000 lbs., what would have ensued if it had followed a different path through space? Might not the increase have been nearly similar in amount in describing any orbit of equal length?

Mr. Greg's elaborate comparisons, indeed, prove meteoric falls to be less frequent at the time of perihelion than at the time of

* See Mädler's "Astronomie" in Bädeker's 'Collective Publication,' vol. iii. p. 595.

† See Mädler, *loc. cit.* p. 629. According to Arago and Herschel, the velocity of our sun in stellar space is only five English miles, or one German mile.—R. P. G.

‡ "Physik und Meteorologie," in Bädeker's 'Collective Publication,' vol. i. p. 169.

§ Probably too large an estimate by three-fourths. See Note at the end of this paper.—R. P. G.

‖ See Nöggerath's "Geologie und Geognosie," in Bädeker's 'Collective Publication,' p. 110.

aphelion. The Sun himself, however, as before shown, far from being stationary, is moving with considerable velocity through stellar space. While the Earth has received an increase of 450,000 lbs., it has completed its orbital movement round the Sun ($2r\pi$, r being the average distance of our globe from the Sun); and in the same space of time, by sharing in the progressive movement of the solar system as a whole, it has run through a space of $11r$. It may be sufficient to admit for the case in question, the approximative expression $r\sqrt{(121+4\pi_2)}$, and even $13r$ (instead of $12{\cdot}65r$). If we compare mutually the space (s) run through by the Earth, and the space (S) run through by the whole solar system in one year (even if we admit for s nearly double the diameter of Neptune's orbit—120 times the Earth's distance from the Sun,—and for the diameter of our globe itself, in round numbers say 2000 [German] miles, far exceeding it, its real value $=0{\cdot}0001r$), we obtain the following numbers:—

$$\begin{aligned} S : s &= 120^2 \times 11 : 13 \times 0{\cdot}0001^2 \\ &= 14{,}400 \times 100{,}100{,}100 \times 11 : 13 \\ &= 1{,}440{,}000{,}000{,}000 \times 11 : 13 \\ &= 15{,}840{,}000{,}000 : 13 \\ &= 1{,}218{,}460{,}000{,}000 : 1. \end{aligned}$$

The space-number of more than one billion, multiplied by 450,000 lbs. (the supposed yearly increase of our globe), gives in pounds the total weight or mass of meteoric matter existing and moving about in every direction within the space above assigned to our solar system. This sum, of over half a trillion of pounds, is, however, not very considerable when compared with the weight of our own globe, calculated to amount to $13\frac{1}{2}$ quadrillions of pounds*. If we suppose these 450,000 lbs. of meteorites to be united into one sphere, the diameter of this sphere would be to the diameter of our globe as 1 to 290·8.

The weight of the terrestrial globe is always to the total weight (450,000 lbs.) of meteorites moving in every direction within the space annually run through by our solar system as 24 millions are to unity. These are then the calculated results arising out of the above-mentioned supposition. A far greater proportion of solid matter distributed into small bodies would be obtained, if we were allowed to take into account the great number of meteors visible within our atmosphere in the shape of shooting-stars, and bolides that do not apparently deposit solid matter, and whose light is probably developed by compression of air, or, if not in every case by actual combustion, as supposed by Reichenbach, at least (as regards meteoric iron) after the manner of

* See Nöggerath's "Geologie und Geognosie," in Bädeker's 'Collective Publication,' p. 110.

"Callum's drops or globules." Professor T. H. Newton of Yale College, New Haven, Conn. U.S., says (*New York Tribune*, August 22, 1860), "it is calculated from perfectly reliable observation, that not less than 10 millions of meteors enter the atmosphere every day, and are burnt up*."

This would then be 3650 millions per annum, which would again materially increase the total amount of meteoric matter contained within the above-mentioned space. But is there not some probability that beyond our own system of fixed stars, all space is replete with such bodies, of which only a proportional *minimum*, and that transitorily, make up part of our own solar system? Not that all of them may be burned away or melted; for the large 4 lb. stone of Segowlee, described by me, has its edges quite sharp and nearly unaltered, some of them being rounded only to a depth of not quite one-twelfth of an inch. Meteorites composed of less dense matter, while moving rapidly, may be frequently again repelled into space by the resistance of compressed air. For this reason meteorites of earthy or carbonaceous-like consistence, as those of Bokkeveld, Alais, &c., are of particular importance, as well as rare. Meteorites are far behind our terrestrial rocks with regard to diversity of mineralogical character. The minerals composing granite, gneiss, mica-schist, and others, representing the most solid basis of the terrestrial crust, are wanting in them; and, to name a particularly important species, they are totally destitute of pure silica or quartz†.

* If, as I presume he does, Prof. Newton means that this represents the total number of meteors which enter our atmosphere daily, and that all these are, as a matter of course, consumed in it, I think he is mistaken, the number of meteors so consumed being in all probability limited to such only as burst or become dissipated in sparks. These form but a small portion of those that apparently fly almost instantaneously through the upper strata of the air (at an average height of 65 miles, as recently proved by Prof. Secchi at Rome), and pass off again, perhaps tangentially, into interplanetary space. Prof. Vaughan of Cincinnati, U.S., thinks it probable that the solid nucleus of an ordinary shooting-star is no bigger than a hailstone; and this is only analogous to what takes place with large aërolitic meteors. There are cases of large and well-observed meteors, which after bursting, sometimes even with noise, into two or more parts at a height of 40 miles or so, have undoubtedly again passed into planetary space, —another proof also that the smaller shooting-stars may do the same.

It must not be forgotten in these rather speculative calculations, that the same groups of meteors may frequently repeat themselves, that is, return periodically without visible or material loss, and that, in fact, by far the greater number of meteors seen are doubtless those that are *periodical* and consequently belong to our own system. Unless it can be otherwise proved, it would seem premature to suppose otherwise than that by far the larger portion of meteors and meteorites of all kinds belong to the solar system, and not to stellar space.—R. P. G.

† Since these lines were written, my highly respected friend Prof. G.

The subject of progressive changes in the world of meteorites, as hitherto exemplified by specimens within our knowledge, is thus for the present confined within narrow limits. Are these progressive changes of a character likely to terminate the proper existence of a celestial body by its definitive division into fragments? or is the possibility of such a "breaking up" justified by any precedent not opposed to the laws of nature as known to us? A few considerations made from this point of view may serve to supply a real *desideratum*.

I intend to sketch them here as briefly as possible, taking my point of departure from a *septaria*, whose constitution I have already explained. As in such a terrestrial concretion, so the outward crust of a cosmical body may become solid, presenting a stony appearance, under the centripetal pressure of gravitation, long before its interior has undergone a like degree of compression. I take our own globe as a point of comparison for data expressed in figures. Originally the particles of the solid terrestrial crust lying next each other may possibly have enjoyed a certain amount of mobility; this of course no longer exists. The maximum of pressure has its seat at a depth where the greater and more solid mass rests on the interior compressed by it in a descending direction. We are entitled to suppose this underlying mass is maintained by this very pressure in a state of incandescent fusion. Atmospheric pressure represented by the weight of a column of water 32 feet high, amounts to 1804·8 lbs. per square foot. A column of 10 feet average height of any substance whose specific gravity is $= 3{\cdot}0$, acts nearly with the same degree of force. At the height of 1 German mile (24,000 feet) the pressure is $=2400$ atmospheres; at 5 miles (25 miles English) (a measure generally adopted to express the solid terrestrial crust*) it would amount to no less than 12,000 atmospheres. A solid pressing on our globe with the weight of 1 lb. would in the Moon press only with $\frac{2}{13}$ lb., and if transported on to the Sun's surface with $28\frac{1}{3}$ lbs.† The pressure produced on

Rose of Berlin, has proved beyond doubt the occurrence of *quartz* in isolated crystals in the meteoric iron of Ziquipilco (Toluca).—W. H.

It may be here mentioned that Berzelius, Rammelsberg, and Dr. Laurence Smith have pointed out strongly and with much truth, the general resemblance that meteorites, in whole or part, not unfrequently bear to certain *volcanic* rocks. See Dr. Buchner's work, *Die Feuermeteore insbesondere Meteoriten, &c.*, p. 175.—R. P. G.

* Important and more recent researches on the question of the thickness of the earth's crust, as conducted by Professor Hopkins and others in this country, may necessitate our raising M. Haidinger's estimate of only 25 miles of a solid crust to something like a *minimum* of 300 miles. This is more a question of degree, however, and does not materially affect M. Haidinger's line of argument.—R. P. G.

† Mädler, *loc. cit.* pp. 577 and 556.

our globe by a solid crust of 5 German miles thick, would require in the Moon a crust of $32\frac{1}{2}$ miles in thickness, and in the Sun a crust of only $\frac{3}{17}$ of a mile thick, or 4235 feet.

Original pressure takes place only so long as a body is not completely solidified; from that moment perfect equilibrium is established within it; pressure, however, must again take place whenever change of temperature modifies the state of rigidity. A rigid body is always apt to conduct heat. That kind of heat whose laws of increase we have to deal with while pursuing investigations on the central heat of the Earth, is *conducted* heat, transmitted from heat generated, or existing, at greater depths, as more immediately shown by volcanic eruptions. In the regions where volcanic vents open in great numbers on the surface, "the smelted interior of our planet," as Humboldt emphatically says, "stands most in permanent communication with the atmosphere." In our times this region is a zone between 75° W. and 125° E. long. of Paris, and 47° S. to 66° N. lat., running N.W. in the western portion of the Southern Ocean*. It deserves consideration, that the whole continent of the Old World lies westward of this zone, separated from it towards the south-west only by the Indian Ocean, offering eastwards (as in the Southern Ocean itself) considerable "areas of subsidence†," and that towards the east of the Southern Ocean the American continent is again fringed with a series of active volcanoes. A remarkable connexion exists between these circumstances and the fact that the altitude of atmospheric strata at the same time rapidly decreases as we approach the antarctic pole,—just as though a mass of solid highlands had in those parts pressed on the interior of the globe at some early period of its existence, and the terrestrial crust had been broken, and its parts mutually dislocated into the general and more marked outlines now visible on its surface. Should the solidification of the crust proceed so far as to become stationary before the particles of primitive cosmical matter enclosed in it have completed their approximation, these might indeed commence a new and independent process of formation, giving rise to a second shell concentric with the first or external one, and enclosing another internal focus of volcanic activity, the primitive one having become meanwhile extinct.

If the sum of 65 miles, expressing the thickness of the Moon's crust taken double, is subtracted from her diameter ($=\frac{264}{1000}$ of the Earth's), and with a density of about 3·37‡,

* *Physical. und geognost. Erinnerungen. Reise der Novara um die Erde*, p. 20.

† See Darwin's 'Theory of the Formation of Coral Reefs.' Humboldt, *loc. cit.*

‡ Arago's 'Popular Astronomy,' translated by Hankel, vol. iv. p. 35.

there would, at all events, remain an interior space of 403 miles, within which the formation of another such spherical shell might possibly proceed. Nevertheless it is not to be expected that further condensation out of the primitive molecular state should go on without some disturbance in a medium of such a temperature as prevails in planetary space. If contraction produces an actual internal vacuum, a *violent disruption* of the crust falls within the bounds of possibility. Admitting that a compensation of temperature by conduction or communication of heat to have already taken place, and supposing every solid shell to be hermetically sealed under a high temperature, an event quite opposite to the above-mentioned one might be expected with some degree of probability. Gases developed within this shell and brought to high tension, might indeed cause a violent explosion, exactly like that arising from ignited gunpowder enclosed within a hollow projectile.

What is the actual cause of the densities of the planetary bodies within our solar system being so different from each other? Does it merely arise from the natural correlation of the elements composing them, as in our globe, or from a progressive development in the earlier stages of their existence? The densities of these bodies are expressed by the following numbers:—Mercury, 6·71; Earth, 5·44; Mars, 5·15; Venus, 5·02; the Moon, 3·37; Sun, 1·37; Jupiter, 1·29; Neptune, 1·2; Uranus, 0·98; Saturn, 1·75.

Olbers is known to have first enounced the hypothesis that the minor planets Ceres and Pallas, discovered by Piazzi and himself, were probably mere fragments of a pre-existing and larger planet. After the discovery of Juno and Vesta, Lagrange* investigated the intensity of an explosive force sufficient to rend a planet into pieces, in order to permit a fragment of it to become a comet, or, to use a more accurate expression, move in an orbit similar to a comet. He found that an impulsion equal to the velocity of a cannon-ball multiplied by 12–15, that is 16,800–21,000 feet per second (the velocity of a cannon-ball being 1400 feet a second, and equal to that of a point at the equator in its diurnal rotation), would be sufficient to throw the fragments of a planet (the radius of its orbit being supposed to be equal to the distance of our globe from the Sun multiplied by 100) into progressive or retrogressive, elliptical or parabolical comet-orbits—the greater number of them even into hyperbolical ones, so that, after their first perihelion, they would disappear for ever from our system†.

* "Sur l'Origine des Comètes." Lu au Bureau des Longitudes, le 29 Janvier, 1812.—*Connaissance des Tems, &c.* pour l'an 1814, Avril 1812, p. 211.

† Baron Reichenbach has expressed an opinion that meteorites may

Certainly there is great difficulty in forming an idea where and how fragments of genuine solid rocks (as meteorites undoubtedly are) could be first violently broken from their parent repository and then hurled into distant solar systems; nevertheless their characteristic fragmentary form, together with the cosmical velocity of their course, leaves no room for any other solution. So daring a supposition, paying, however, due attention to Nature's laws as far as they are known to us, must, however, from time to time, provoke reiterated criticism.

I thought it desirable therefore to give here a short conspectus of such views concerning meteoric phenomena as have from time to time crossed my mind, or been the subject of distinct communications to the Academy, though at the same time I freely admit I may have been intruding into a region of natural science for the investigation of which I am but very imperfectly prepared. I must ask for some indulgence in this attempt to trace the outlines of views in some way different from current ones—the more so since they are intended to establish merely a kind of programme for more accurate investigations.

In an earlier period of development in human society, the "*nonum prematur in annum*" may have been more easily obeyed than it is in our times. Accelerated publication, however, has also its advantages, as contemporaneous investigators familiarized with the matter find in it a point of comparison for their own either analogous or contradictory views. For myself, some portion or other of my views have been more than once the subject of conversation and epistolary intercourse.

At least I hope I may have been successful in my endeavours to follow the strict rules of scientific induction for arriving at the result aimed at in this paper. During the whole course of these considerations I have made it my duty implicitly to obey the precept of our great master, Humboldt, that, "even within merely conjectural regions, uncontroled or arbitrary opinions, independent of induction, should never be allowed to prevail."

Note.—Baron Reichenbach's estimate of an annual meteoric deposit on the surface of our globe, amounting to 450,000 (Vienna) pounds, is certainly considerably over the mark. In his paper on this

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originally have been condensed from comet-dust; that this is quite contrary to M. Haidinger's opinion, I have good reason to believe. It may perhaps appear a little difficult to believe that, were any small planet or satellite of a planet to burst, some of the fragments would for ever be hurled beyond the influence of the sun,—though, as in the case possibly of the sixty asteroids, the original orbital conditions of the parent mass might become a good deal modified.—R. P. G.

subject (see Poggendorff's *Annalen*, vol. cv. p. 554 *et seq.*) he calculates there are 4500 meteoric falls per annum, averaging 100 lbs. per fall in weight. Assuming, as we perhaps may do, that he has not materially over-estimated the weight of each fall, he has certainly exaggerated their annual number. Supposing, in the first place, as I believe we may, that detonating meteors are equally aërolitic, whether stones are picked up or not, since most meteoric stones have resulted from a detonating meteor in the first instance, then for the last sixty years, over an area of 900,000 square miles, comprising the countries of Great Britain and Ireland, France, Germany (inclusive of Austria, Prussia, Hungary, &c.), and Italy, we find recorded (see my "Catalogue of Meteorites and Meteors" in the last volume of the British Association Report for the Oxford Meeting, p. 48) *sixty-nine* actual stone-falls, and *seventy-two* meteors accompanied with detonations from which no material *residuum* was obtained; say in all 144 cases of aërolitic phenomena. That is about $2\frac{1}{3}$ recorded instances per annum for an area of 900,000 square miles; and taking the superficial area of the whole globe at 197,000,000 square miles, we obtain rather over *five hundred* falls (511) as the number likely to be observed, were all the world covered by land and peopled in like manner by Europeans.

Now, what proportion this number would bear to those that absolutely do fall annually, but which are never noticed or not recorded in scientific works, it is not very easy to say; but from various reasons it may be fairly estimated at more than half of the entire number. Chladni and Humboldt have estimated the total number at 700.

There are several reasons for inducing us to increase the annual number of observed and recorded falls, viz. 500, to 800 or 900, as the *actual* number that fall, and not more. First, the fact that one-half the human race are supposed to be asleep or in their houses nearly twelve hours out of the twenty-four, must tend to limit considerably the number of observations; on the other hand, we are not without instances of stone-falls and other aërolitic phenomena, detonating meteors more especially, occurring during the night-time; while again, as I have shown in the tabulated results of my large Catalogue, p. 118, the greatest number of stone-falls seem to occur in the afternoon about 4 o'clock, not only as against falls taking place during the night, but as compared even with the corresponding hours in the forenoon, equally favourable as a time for such observations. Though stones have not frequently been picked up during the night-time, we may bear in mind that the night is a most favourable time for seeing large and brilliant aërolitic meteors, and that the darkness does not prevent us from hearing the violent detonations usually accompanying the explosion of an aërolitic meteor. Then, again, it is not unusual for an aërolitic meteor to pass overhead some hundreds of miles, and for the detonation to be heard over from twenty to forty miles square; and some persons would probably notice one or the other. Now as I have included as *aërolitic*, meteors from which no stones have been picked up, it will I think be admitted that to double the entire number of both classes actually recorded

in catalogues drawn from every available source is a reasonable estimate, especially when based on observations made in civilized and densely peopled countries like England, France, and Germany. How few persons, I may also add, are there who have ever in their lifetime either seen a meteoric stone fall, or even heard the always violent detonation of an exploding aërolitic meteor. There seems to have been of the latter only three instances recorded as observed in England during the last ten years, and no well-authenticated instance of a meteoric-stone fall since 1835, and that a single one of about 2 lbs. in weight! To know whether a meteoric stone has fallen, it is then not exactly necessary to calculate the proportions of waste or forest ground, &c. that exists even in Europe, as Baron Reichenbach argues, in order to arrive at the number of stones *not* picked up, if we assume that aërolitic detonating meteors are seen and heard as a rule over very large areas, and count as actual falls in our calculations. So striking indeed are the phenomena usually attending the fall and appearance of aërolites and aërolitic meteors, that I much question whether fully *two-thirds* of the real number would not certainly be recorded in the daily or scientific journals, say of England and France. Instead, therefore, of placing the total weight of meteoric matter annually deposited on the earth as high as 450,000 lbs., as calculated by Baron Reichenbach, I am inclined to estimate it at probably less than 100,000 lbs. This is, however, more a question of degree, and does not vitally affect the ulterior argument involved in the problem proposed by Baron Reichenbach and M. Haidinger—a problem not without importance and interest, though somewhat speculative.—R.P.G.

The reader is requested to correct the following errata in the first portion of this paper:—

Page 353, line 10 from top, *for* 24,000 *read* 24,000×7.
— — line 11 from top, *for* 124·4 *read* 1247.
— — line 14 from top, *for* 4080·32 *read* 40901·6.

LVIII. *A Sketch of* M. Faye's "*Examen d'un Mémoire de M. Plana sur la force répulsive et le milieu résistant*," *with a few Remarks thereon. By* Henry S. Boase, *M.D., F.R.S. & G.S.**

THE concluding summary of M. Faye's memoir, published in the 'Illustrated London News' for September 7 under the head of "Scientific News," attracted my attention, and excited a strong desire to see the memoir itself; for its statements concerning the duality and universality of the forces attraction and repulsion, in celestial phenomena, seemed to indicate views very similar to those advanced in my 'Philosophy of Nature.' Having

* Communicated by the Author.

since read this memoir in the August Number of the *Comptes Rendus* of the French Academy, I found that there was not the slightest foundation for the supposed similarity of opinions; but its contents are so interesting, that I was induced to make copious extracts therefrom: and I am inclined to think that they will be generally acceptable; for even those most averse to speculations cannot refuse their attention to the opinions of such an illustrious man.

M. Faye commences his memoir by briefly and clearly stating the points of the thesis which he proposes to prove, which are,—1st, that the hypothesis of a resisting medium, as formulated by geometers, is unacceptable; 2nd, that if it be corrected so as to render it more rational, it becomes too indefinite for directing analysis; 3rd, that the theory of the repulsive force is the only one that can be scientifically constituted.

The hypothesis of a resisting medium, says M. Faye, applies very well as far as the motions of the periodical comets of three and seven years; it gives a clear and precise idea of the nature of their acceleration. According to M. Plana, this theory gives $29''{\cdot}5$ for the variation of the second comet's eccentricity, a result very near $34''{\cdot}6$ assigned by observation. Since M. Plana, M. Axel Möller advances a step further by introducing, after the suggestion of M. Valz, the variation of volume which a comet, supposing it compressible and not permeable to the surrounding medium, ought to experience when it penetrates into the gradually denser strata of this medium. The diminution of eccentricity is then equal to $32''$, that is, almost identical with the result of observation. But could we approach still nearer to $34''{\cdot}6$, the value of which is not indeed definitively fixed, I would still not the less persist in regarding these formulæ and calculations as purely empirical, inasmuch as it has not yet been proved that a ponderable medium, elastic or not, can exist around the sun without revolving round it.

This idea of a heavy (or gravitating) and immoveable medium, says M. Faye, is no novelty; it may be traced back to the *materia cœlorum* of the ancients, which was supposed to fill the world after the manner of the extension of the atmosphere of a central body; and since the time of Newton, its existence has only been upheld for the purpose of conserving the conception of gravitation, as the one governing force of the universe. But this singular hypothesis ought to have vanished when Laplace made known the definite limits which mechanics imposes on the atmospheres of celestial bodies.

In vain we suppose, in order to evade the objection, that this medium is imponderable; for then we must have recourse to the æther of physicists. But in this case we must no longer attri-

bute to the medium a density proportional to $\frac{1}{r^2}$, because in ceasing to gravitate towards the sun, the beds of this medium will cease to mutually press on one another in that direction; it therefore becomes requisite to attribute to the medium a constant density in space. Unfortunately, in this case, the beautiful agreement above alluded to between calculation and observation will disappear; for instead of 34″·6, the formula of M. Plana for this hypothesis will not give more than 14″; and, moreover, there still remains to apply to this medium a velocity of translation equal and contrary to that which transports the masses of the solar system in indefinite space.

It is a long time ago, says M. Faye, since I first advanced this objection: no one has ever been able to answer it; and, notwithstanding, the hypothesis of an immoveable medium is persisted in. Can it be then that in itself the immobility or the movement of the medium is a thing of no consequence? This is what we are going to see.

Let us accept, then, continues M. Faye, the very different hypothesis of a medium revolving around the sun. It is then evident that such a medium can only resist the motion of comets in virtue of the excess of velocity. This excess, positive at perihelion, becomes negative at aphelion; consequently if the medium resists in the one case, it will push in the other. This circumstance alone is sufficient to show that the analysis of this new problem cannot be identified with that of the first. It indeed involves more than this; for what then will become of the law of density? No one can tell. When the medium was regarded as immoveable, it was admitted that its beds, in gravitating the one on the other and on the sun, would be mutually compressed in such a manner that the density would progressively increase towards the interior, according to the law approximately represented by $\frac{1}{r^2}$. But when the medium revolves, it ceases to gravitate, not only towards, but on the sun; its beds cease to press upon each other and the law of density becomes a perfectly indefinite problem.

M. Faye then gives some analyses for the periodical comet of seven years which bears his name; the result of which is 2′ 40″, instead of 34″·6 as given by observation. From this enormous discordance, he says, it must be concluded that a continuous ring of constant density is inadmissible. It is necessary, therefore, that the density of the medium should vary according to a peculiar law. Thus Encke's comet requires that this density should go on rapidly decreasing outside the orbit of Mercury,

within which orbit the perihelion of this comet occurs. Again, the comet of seven years likewise requires, no less imperiously, that the density of such a ring should be well marked in the region of the orbit of Mars, rapidly decreasing in such a manner as to become imperceptible on approaching the orbit of Jupiter; for it is between these orbits that the motions of this remarkable comet are accomplished. These conditions can only be reconciled by adopting for the resisting medium a series of cosmical rings more or less resembling those of Saturn, but separated from one another by great intervals.

Such, in the opinion of M. Faye, is the only form under which the hypothesis of a resisting medium can be hereafter maintained. But he adds that nothing can be more indefinite than such a hypothesis; for the number of such rings, their respective limits, and the law of their interior density are completely arbitrary. It is impossible, for example, to extract from it any relation between δn, or $\delta\zeta$ and $\delta\phi$, on which, however, all the memoir of M. Plana proceeds.

In this manner, says M. Faye, the two first parts of his thesis have been justified: it has been shown that an immoveable medium is impossible; that a revolving medium is an indefinite hypothesis with which analysis can have no concern; and that a series of cosmical rings is so fanciful that such a hypothesis must be ranked with the transparent crystalline heavens and with the Cartesian vortices. And he lastly proceeds to the consideration of his theory of a repulsive force.

The several successive returns of the comet of three years have taught us that the duration of its revolution is constantly diminishing, whilst the other elements of its orbit remain unaltered. This is a most important fact; and Encke, the author of this great discovery, has concluded therefrom the existence of a force which is constantly opposed to the motion of the comet, and which therefore results in the comet's successive acceleration, without affecting its other elements, save the eccentricity. But this repulsive tangential force, is it real or apparent? If it be a real force, it may be asked what such a force can be which is able to contend in the heavens with gravitation, and thus to break the unity of astronomical science. Encke has declared for an apparent force, originating in the resistance of a medium; it was, it may be supposed, merely for the conservation of this threatened unity. It is doubtless an enlightened view, but arbitrary; for the unity of force is not, it is presumed, a scientific dogma. Let us then reserve our judgment concerning the nature of this repulsive force.

In studying, says M. Faye, the astonishing forms which comets present, their gigantic tails, the matter which they seem

to dart towards the sun, but which soon returns to be confounded with the tail, &c., all the world will naturally say that such things come to pass because the sun exercises a repulsive action upon the atmosphere of the comets. Some will have it that this repulsive action is due to electricity, and others to magnetism, without once reflecting that these forces, so precise when applied to terrestrial phenomena, are vague and little understood in relation to celestial bodies. Others, again, have spoken of an apparent repulsion; it was the idea of Hooke and of Newton. Bessel, after a very profound study of certain phenomena, which he has, however, too much generalized, can only see in these cometary forms the effects of polar forces analogous to magnetism. But to form an opinion concerning the nature of such a force, one single order of facts is not sufficient; for it entails the necessity of guessing. We therefore again ask, what is this repulsive force?

Such was the state of the case, M. Faye says, when he took up the question:—on one side a repulsive tangential force, indicated by the motions of comets; on the other a radial repulsive force, indicated by their tails: on one hand, Encke with the ancient hypothesis of a resisting medium for the explanation of the former force; on the other, Bessel with his polar forces to account for the latter. A discussion, short but memorable, took place between these great astronomers. Bessel, who did not believe more than myself in a resisting medium, referred every thing to his radial forces; Encke, on the other hand, pointed out to him that this was impossible. So it would seem that the resisting medium adopted by the one is a physical impossibility; the play of forces, imagined by the other, in consideration of a single fact arbitrarily generalized, is still more inadmissible.

These two forces, real or apparent, are both repulsive; can they be reduced to a single force? If any celestial body exercise this singular action, it can be no other than the sun itself. But can it be imagined that a force emanating from the sun can act on any body in any other direction than that of the radius vector? Yes, certainly, the mechanician answers, if the body moves from right to left, and if the force is not instantaneously propagated like that of gravitation, but with an enormous velocity, indicated by the disproportion of the composing forces. So, then, the repulsive force exercised by the sun and endowed with a successive propagation, after the manner of luminous and calorific radiations, will furnish the two composing forces, the one radial and the other tangential, which are required to explain both the forms and the motions of comets. In studying the radial force under this point of view, it will be readily seen that this ought to be a force independent of the mass, and propor-

tional to the extent of surface. The tangential force of this composition leads to precisely the same conclusions. The sun alone exercises it: it does not depend on the sun's mass, which is not operative in this case; it is probably only caused by the incandescence of the sun's surface; for this it is that distinguishes the sun from the planets, the vicinity of which does not affect the figure of the comets.

Such is an idea of the repulsive-force hypothesis, so far developed; a few steps more, and its astronomical formula may be attained. A repulsive force operating at all distances, but evidently becoming more feeble, and that rapidly as the distance increases; emanating from an incandescent polar surface; propagated with a velocity equal to that of radiant caloric; in the ratio of the surface and not of the mass; and pressing on the surfaces which it repels, in place of acting through all matter like gravitation:—Is there such a force in Nature?

If such a force exists, says M. Faye, it is probably the same repulsive force which is manifested in all material bodies under the names of dilatation, of expansion, of elasticity, &c. As in the case of the assumed astronomical force, the physical force which presides over these phenomena is due to heat; like it, its action is repulsive, does not extend through bodies, decreases rapidly with the distance, and relates to the surface and not to the mass; but there is a single difference between them: the physical, unlike the astronomical force, cannot act at a distance; so say many physicists; beyond molecular intervals it is imperceptible. But this, M. Faye, in common with Fresnel and others, regards only as an *à priori* opinion, not justified by experimental research: it ought to be proved that calorific repulsion, which acts from molecule to molecule in solids, liquids, and gases, that is to say, at intervals successively increasing at an enormous ratio, does not suddenly vanish at certain particular distances.

M. Faye says that for nearly three years he has worked on this subject; and in order to remind the Academy, he rapidly recounts the results up to the present time, viz. that his theory accounts for all the observed phenomena concerning the acceleration of cometary motions. Hereafter account must be taken of the variation of volume in the nucleus of comets, which doubtless will present some difficulties. He has also shown that, varied as the figures of comets are (which are complicated by the effects of perspective), they possess certain features in common—features which in their unity constitute in some measure a normal figure, which can be separated from accidental differences. And he thinks that the coexistence of several tails may be simply explained by the presence of substances having dif-

ferent specific gravities in the atmospheres of comets: the repulsive action of the sun, operating on them after the manner of the metallurgical washings of ores, would arrange them in trains more or less curved in the rear of the general motion according to the greater density of these substances. The repulsive force exercised by the sun on substances reduced to a great tenuity in cometary nebulosities, also explains the most general and important facts concerning the curvature of their tails, of their multiplicity, and of their form, not conical, as Arago supposed, but flat and fully displayed in the plane of their orbit, &c. The difficulty which so long attended the study of their heads and their atmospheres has disappeared as by enchantment.

In conducting his experiments in verification of the repulsive force, M. Faye was guided by the memorable experiment of Cavendish on the mutual attraction between two solid bodies; in which the scientific world, he says, did not see so much the indispensable confirmation of the Newtonian theory, as an ingenious method of ascertaining the density of the earth. In experimenting, however, on the repulsive force, the question assumes a different aspect, as the earth *no longer* exercises this force in a sensible manner; for at the present day the sun alone possesses this property, since it alone, in our little world, has preserved its primeval incandescence. Nor can we experiment on the solar action, because its repulsive force does not come within our reach; it is dissipated on the superior beds of the atmosphere, in consequence of its incapacity of acting through all matter after the manner of gravitation. So we are reduced to study the feeble forces which can be produced by artificial means. In experimenting on the attraction between two bodies, the influence of the air and of its currents is a great obstacle to success; and the same influence is still more obstructive in the case of the repulsive force, since one of the bodies employed requires to be in a state of incandescence. This is the reason why all the attempts have miscarried up to the present time. He then refers to the arrangements which he adopted for surmounting these difficulties, and announces a new series of experiments in hand, the incandescence being produced by the voltaic current in a vacuum rendered more perfect by chemical action; and he hopes to be able, not only to demonstrate repulsion at a distance, but also to measure it.

Lastly, M. Faye thus sums up the conclusions at which he has arrived by his labours. The celestial world does not obey one force alone, attraction, but a duality of forces, attraction and repulsion. The former depends solely on the mass, the latter on the surface and heat. The one is propagated instantaneously, the other successively. The one acts through all matter

without suffering any diminution, the other is intercepted even by a simple screen. Both these forces are universal; for they occur everywhere throughout the system of Nature.

Having at length finished the sketch of M. Faye's memoir, it is now proposed to make a few remarks thereon: and these remarks will principally apply to two points—an argument in favour of the existence of a resisting medium, the æther of space, and some objections to the proposed repulsive force.

But before proceeding, it may not be out of place to notice that the appearance of speculations from such an illustrious man, and in such a distinguished publication, is another notable sign of the times, and proves that there is a growing desire in the scientific world to emancipate itself from that thraldom to the accumulation of facts which in the first instance is wise, if only the means to an end, but which, if dogmatically adhered to as intrinsically valuable in itself, can never enlarge the field of science. It is like the folly of the miser who accumulates money, but never applies it to its legitimate use. Facts we must have, they are the raw material of science; but a good hypothesis is of more value than a multitude of similar facts. To the same effect M. Faye observes that his hypothesis has already proved to be fruitful, and that it will in the hands of practical men render much greater services; for to observe well, a good theory is almost as necessary as a good telescope. Acting on this principle, how much Laplace has accomplished in France; and in this country Newton has done more for science than any other man by his theory of gravitation, which was not attained by personal industry in accumulating facts, but by pondering on known facts till utilized by generalization; for, as he says in writing to the astronomer Flamsteed, "all the world knows that I make no observations myself."

It would seem that the great difficulty in accepting the existence of a resisting medium depends on the discrepancy between the calculated and the observed amount of the variation in the eccentricity of Encke and Faye's comets: but are the data of these calculations so perfectly reliable, that the failure in the analysis can subvert the established opinion concerning the nature of the æther of space? If it be ignored, what becomes of the beautiful undulatory theory which explains the progression of light and radiant heat? It is difficult to conceive that any physical force, including gravitating force, can be transferred from one part of the solar system to another without a successive action and reaction on intermediate parts. In times past the *actio in distans* was accepted because the phenomenon viewed only superficially presented this appearance; and to revert to such a doctrine, unless supported by indisputable facts, can be

no other than a retrograde movement in science. If any force can be said to act at a distance, it surely may be supposed to be that of electricity, for it has all the outward appearance of such an action; but Faraday's elaborate and searching inquiry into all the obscure conditions of the case has established beyond dispute that there is a communication of this force from molecule to molecule by the process of induction. M. Faye need not search after a perfect vacuum in which to perform experiments for proving the action of repulsive force at a distance; for the sodium he employs in removing the residue of oxygen after mechanical exhaustion will fill it with highly elastic vapour, the presence of which may be demonstrated by an electric discharge; and indeed the very electric incandescence will also fill the vessel with the vapour of the metallic poles, to say nothing of the æther of space, for this is the point in dispute; but surely if that æther be the means of transmitting radiant heat from the sun to all its planets, it must also be sufficient within an exhausted vessel. It does not, however, very clearly appear that M. Faye directly disputes luminous and calorific radiations; for he says that his radial repulsive force is exercised in the same manner, and that it is in all its properties identical with physical heat. If it be that which radiates from incandescent surfaces, it can be no other than radiant heat; it only remains to decide its mode of operation through space and other diathermanous media. Newton was decidedly opposed to the *actio in distans*; for in his third letter to Bentley he observes, "That one body may act upon another at a distance through a *vacuum* without the mediation of anything else, by and through which their action and force may be conveyed from one to another, is to me a great absurdity."

In the pursuit of science, the only legitimate method is to proceed from the known to the unknown. We know that in the case of a series of suspended ivory balls, the ball at one extremity will be put in motion by raising and letting fall the ball at the other extremity: here we have a visible and tangible medium through which force is transmitted, and by removal of which the extreme balls cannot affect each other. So in the electric telegraph, we know that force applied at one end of the conducting wire will produce motion at the other, but its transmission depends on the presence of the intervening medium. When, therefore, in experiments on radiant heat, we find that this force will affect bodies at a distance from its source, even through diathermanous screens, it is a legitimate inference that this must also take place through an appropriate medium, even though it be not tangible or visible. Such a medium has been acknowledged as adequate to the transmission of light by the

vibrations of its atoms; and if so, it is capable of performing the same office for radiant heat, which always accompanies light in the sunbeam. Surely the admission of the universal presence of the æther of space as a medium for the transmission of physical forces, supported as it is by many facts that such forces are so propagated, is preferable to the bare assumption that, because celestial attraction and repulsion *appear* to act at a distance, therefore it is a fact that they do so act. It is clearly beginning at the wrong end: this question relates to the simple fact, the *an sit* of Aristotle; and in such a case no assumption is justifiable.

M. Faye's analyses relating to the acceleration of comets, seem to have rested on two data concerning a resisting medium: in the one case the resisting medium is regarded as gravitating but immoveable; in the other case as an imponderable (or immaterial) and revolving medium.

It does not appear in this memoir why such a distinction was adopted; and it is not easy to imagine the grounds for such conceptions concerning the æther of space. If it be a gravitating material fluid, it must necessarily partake of the common motion of the entire solar system; and if so, it remains to revise the data of the calculations, the resisting medium not being stationary, but spirally revolving around the sun with a velocity progressively increasing from the confines to the centre of the solar system. "When the medium revolves," says M. Faye, "it ceases to gravitate towards the sun, and its layers also cease to press each other according to the law of density." This statement is incomprehensible: atmospheres revolve with their respective central bodies, as æther with the sun, and yet their layers press on each other. If the medium possess any weight, however inappreciable, it cannot lose this by revolving; and if it be imponderable or immaterial, as the older physicists considered the æther to be, it could not revolve; for motion depends on a composition of forces essentially material or physical, that is, of attraction and repulsion, to the former of which the weight of bodies is due.

From these considerations it is evident that the notion concerning the nature of æther held by our neighbours is very different from that which we entertain. Grove thinks (and most of us agree with him) that æther is a highly elastic fluid having weight, though the amount of it is beyond the reach of determination, its excessive tenuity rendering it inappreciable. He regards this matter as the rarefied extensions of the atmospheres of celestial bodies. In my 'Philosophy of Nature' a different view is advanced; but we both agree in its being a material elastic fluid; and if so, it must follow the common law of fluids, and be more and

more condensed in the vicinity of every celestial mass, and more particularly around the sun, which in mass transcends all the others. In calculating the amount of this density as progressively increasing towards the centre of the system, the intense heat of the sun need not be regarded as a modifying force; for its rays can in no way affect the æther, since it is perfectly diathermanous.

The grand objection to the æther as a resisting medium adequate to the explanation of cometary acceleration, is the statement of M. Faye, that each comet would require a distinct zone or ring in space, varying in density not regularly in the ratio of the sun's distance, but sometimes inversely, as in the case of some of Saturn's rings. Should this be established as a fact, some other disturbing influences must be sought for besides æther as a resisting medium; but still this would not annul the existence of æther, for it would ever remain a *datum* as a retarding influence *quantum valeat.*

In such a wide field for speculation, it is not difficult to imagine that such rings may exist in space as cosmical or nebular matter of extreme tenuity and yet really ponderous as compared with the æther in which they are situated. Such matter may be the residue of the solar zones, from which, according to Laplace's theory, each planet was evolved; or, a new form assumed by comets, which by the successive shortening of the major axis of their orbits, have been reduced to their mean distance from the sun, at which place the body is still very voluminous, and would be still more rarefied if reduced to the state of a ring revolving around the sun as in the case of Saturn.

Before concluding, a few words may be said concerning the operation of a repulsive force emanating from the sun, and which is said to be sufficient for the explanation of all the phenomena of comets, including the acceleration of the periodical comets: yet in truth we know but little concerning these phenomena, nothing concerning their rotation, although, as revolving bodies, they doubtless do rotate.

The radiant heat issuing from the incandescent surface of the sun is certainly the *vera causa* of many calorific phenomena; but that it can accelerate directly or indirectly the motion of a comet does not seem probable. A comet falling towards the sun with enormously increasing velocity, like the return of a rocket to the earth, will *pari passu* contract in volume, and consequently increase in density, in consequence of the increased influence of gravitation: when it has gained its perihelion, like a vibrating pendulum, it will be carried by its momentum from the sun with gradually diminishing velocity and increasing volume until it attains to its aphelion. If the comet, in its approach to the sun, passes through a resisting medium, especially if revolving in a

direction in any way opposite to its motion, the friction must be tantamount to an increase of gravity, which, by reducing the orbit, must increase the centrifugal force, thereby accelerating its motion. This cause alone may not account for the entire alteration; but the subject is as yet in its infancy, and cannot be fairly condemned in the summary manner in which M. Faye has dealt with it. He gives us, it is true, another hypothesis in its place, but it does not seem to be in any way superior to that of Encke. We know the effect which radiant heat has on bodies when it impinges on them: if the surface be reflecting, accord- to its degree the rays of heat are diverted; if it be absorbing, the heat assumes another phase and operates as an expansive force; if it be diathermanous, it passes freely through and renews its course on the opposite side of the body. In the case of comets, radiant heat can only be supposed to act in the two last-mentioned ways: in the last, in respect to a comet's motion, it must be perfectly inoperative; in the second, a certain amount of heat would be absorbed and enlarge the volume of the comet, thereby *pro tanto* counteracting the condensing effect of the sun's gravitating pressure.

M. Faye would seem to imply that the repulsive force of the sun is arrested by the surface of the comet, and thus is enabled to forcibly act on it as by impact; and agreeably to this notion he asserts, what is contrary to our daily experience, that "the repulsive force of the sun does not come within our reach; that it is dissipated on the superior beds of the atmosphere, in consequence of its incapacity of acting through all matter after the manner of gravitation." If it could be supposed that comets are enveloped by an impervious skin, as M. Valz suggested, and that this, moreover, had a good reflecting surface like some polished metals, then the impinging and reflecting of an enormous multitude of the sun's rays might by their aggregate force produce a sensible motion. But of what avail are suggestions of this kind: hypotheses arrived at inductively from facts may be tolerated, though they may prove to be invalid; but it is perfectly illogical to assume a fact, and then make it the subject of an argument.

It may be that M. Faye's repulsive force emanating from the sun may be well conceived, but imperfectly apprehended: it may prove to be akin to centrifugal force, which is the correlative of gravity, as set forth in the 'Philosophy of Nature.' But then such a mode of repulsion is quite different from that other mode of repulsion, which, as radiant heat, not only emanates from *incandescent*, but also from all *hot* bodies, and which is transmitted from body to body by the vibrations of ethereal atoms.

Each mode of the physical forces can only be manifested by

their phenomena, which are various kinds of motion: of these the principal motions seem to be radiations by undulations, and circuits by polarization. When the former relate to material molecules, the phenomena are best known as sound; when they relate to the medium æther, they become luminous and calorific phenomena: and so likewise the polar actions of molecules are known as electrical and magnetic phenomena; those of æther, as the tangential currents due to gravitating and centrifugal forces. And should the same ratio of velocity as occurs between sound and light hold good between electricity and centrifugal force, and between magnetism and gravitation, the cosmical forces gravity and centrifugy must evidently be instantaneous in their operations, and not successive by undulations like radiant heat and actinism.

Claverhouse, near Dundee,
October 1861.

LIX. *Observations on Lunar Radiation.* *By* Professor Tyndall, *F.R.S.**

I HAD hoped, before the appearance of the present Number of this Magazine, to be able to prosecute the observations on Lunar Radiation referred to in my letter to Sir John Herschel to a definite issue; but I am so closely occupied with inquiries of another kind, that I must for the present content myself with recording the observations on which the remarks contained in the letter referred to were founded.

My place of observation was the roof of the Royal Institution in Albemarle Street, where I had a platform erected, sufficiently high to enable me to sweep a large portion of the heavens with my thermo-electric pile, without impediment from the chimney-pots. Wires were carried from the pile to an excellent galvanometer placed in the laboratory, the floor of which was about seventy-two feet below the platform.

On directing the axis of the pile towards the heavens, the chilling produced by radiation from its exposed face was so considerable†, and the consequent galvanometric deflection so great, that it was quite hopeless to operate on the needle in this position. To move it a single degree would have required many hundred times the quantity of heat or cold necessary to urge it through one of the lower degrees of the galvanometric scale; I therefore operated as follows:—

* Communicated by the Author.

† I intend to make this mixed action of our atmosphere and stellar space the subject of a special investigation. At midday also the refrigeration of the zenith is very great.

The galvanometer was a differential one; that is to say, two wires ran side by side round the astatic needle of the instrument. The ends of one of these wires were connected with the pile on the roof, the ends of the second wire were connected with a second pile, which was turned towards a vessel kept at a constant temperature by boiling water. The direction of the current caused by the heat below was opposed to that generated by the cold above; one of them in a great measure neutralized the other, and the needle was thus compelled to take up its place among the lower degrees of the scale.

I then ascended to the roof, fixed my pile at the proper angle, and directed it *off* the moon; I descended and observed the galvanometer; the needle oscillated between 10° and 20°, its mean position being therefore 15°.

I reascended and turned the pile *on* the moon; on descending I found the needle oscillating between 35° and 45°, the mean position being 40°.

The ascending and descending was repeated six times, and the following results were obtained:—

Mean deflection.	
Off the moon.	On the moon.
°	°
15	40
27	40
33	40

These numbers all show *cold*, the deflection being such as would be produced by the cooling of the face of the pile presented to the heavens; and the result is that *the chilling was in all cases greatest when the pile was directed towards the moon.*

The explanation given of this result in my letter to Sir John Herschel, I think, deals with a true cause. One hot body may, I think, be chilled by the presence of another in virtue of an action on the intervening medium. But whether the cause is *sufficient* may admit of question. It would not be sufficient if the height of our atmosphere were restricted to the limits which many assign to it. But if I understood the Astronomer Royal aright at Manchester, there is some reason for supposing the atmosphere to extend immeasurably beyond those limits. But then its extreme tenuity at great distances would probably be urged against the possibility of its producing any sensible effect. Tenuity in the abstract, however, hardly furnishes a sufficient argument. In a very few weeks I shall have occasion to show that the action of a stratum of vapour three feet thick, and possessing a tenuity which amounts only to a fraction of that assigned to our atmosphere at a height of eighty miles, is capable of accurate mea-

surement. Nevertheless it would be a mere game of intellectual gymnastics to continue such speculations as these; for reflection on observations made before and since the publication of my letter to Sir John Herschel, leads me to conclude that in the atmosphere of London it is perfectly hopeless to obtain trustworthy results on this very delicate question.

For example, my place of observation was Albemarle Street, and my pile when turned on the moon looked nearly due south. The reflector of the instrument thus cleared in a great measure the buildings of Lambeth. I turned the instrument eastward, through a large arc, but in so doing came more over the mass of London. This *may* account for the diminished loss of heat. But even this, though apparently a natural one enough, I should hesitate to assign as the real cause of the result observed. Fresh experiments, under different conditions, will be required to decide the question.

I may add that I have furnished the pile with a conical reflector of polished tin of vast dimensions, hoping thereby to collect, not only the moon's luminous rays, but also her obscure rays, which even if they reached the earth, were effectually cut off by the polyzonal lens which Melloni used in his experiments on the moon. To protect the exposed face of the pile from currents of air, I have had the reflector furnished with screens of rock-salt. But these precautions led to no satisfactory result, the irregularities of the London atmosphere producing disturbances of the galvanometer far more than sufficient to mask the effect of the moon's rays.

LX. *On the Blue Band of the Lithium Spectrum.* By Professor Frankland, *F.R.S.*

Chemical Theatre, St. Bartholomew's Hospital, E.C.,
November 7, 1861.

My dear Tyndall,

ON throwing the spectrum of lithium upon the screen yesterday, I was surprised to see a magnificent blue band. At first I thought the chloride of lithium must be adulterated with strontium; but on testing it with Steinheil's apparatus, it yielded normal results without any trace of a blue band. I am just now reading the report of your Discourse in the 'Chemical News,' and I find that you have noticed the same thing. Whence does this blue line arise? Does it really belong to the lithium, or are the coke-points or ignited air guilty of its production? I find three blue bands with chloride of sodium, but they have not the definiteness and brilliancy of the lithium band. When lithium wire burns in air, it emits a splendid crimson light; plunge it into oxygen, and the light changes to bluish white.

This seems to indicate that a high temperature is necessary to bring out the blue ray.

Ever yours sincerely,

E. Frankland.

P.S. I have just made some further experiments on the lithium spectrum, and they conclusively prove that the appearance of the blue line entirely depends upon temperature. The spectrum of chloride of lithium ignited in a Bunsen's-burner flame does not disclose the faintest trace of the blue line: replace the Bunsen's burner by a jet of hydrogen (the temperature of which is higher than that of the Bunsen's burner), and the blue line appears, faint, it is true, but sharp and quite unmistakeable; if oxygen be now slowly turned into the jet, the brilliancy of the blue line increases until the temperature of the flame rises high enough to fuse the platinum and thus put an end to the experiment.—E. F.

November 22, 1861.

[On the occasion referred to by Dr. Frankland, it was a general impression among the chemists present at the lecture that I had used the word lithium for strontium throughout the evening. This induced me to ask Dr. Miller to test my chloride of lithium, which he found quite pure. I afterwards showed the blue band, the splendour of which is unrivalled, to my class at the School of Mines. The coal-points without the lithium show nothing of the kind; with the lithium the band always appears. Either therefore the substance itself is so altered by the exalted temperature that new periods of oscillation are possible to it, or the medium in which it vibrates is so changed in elasticity as to permit of the same thing. The observation appears to be one of considerable significance. I would also draw attention to the experiment by which the absorption of the yellow band by the sodium flame was effected on the same occasion, as one of the most striking class experiments in the whole range of optics. It is very easily performed, a band 18 inches long and $\frac{3}{4}$ of an inch wide being quite attainable within ordinary lecture-room limits. A salt flame 10 feet thick produced no such effect. Dr. Miller, I am informed, repeated this experiment with success before an evening meeting of the British Association at Manchester (see Phil. Mag. vol. xxii. p. 154).—J. T.]

LXI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 403.]

December 20, 1860.—Major-General Sabine, R.A., Treasurer and Vice-President in the Chair.

THE following communications were read:—

"Researches on the Arsenic-Bases." By A. W. Hofmann, LL.D., F.R.S.

In a previous note* I have shown the existence of a group of dia-

* Phil. Mag. for September, p. 245, "Researches on the Phosphorus-Bases. No. IX. Phospharsonium Compounds."

tomic bases, containing phosphorus and arsenic, which are formed by the action of monarsines on the bromethylated bromide, so frequently mentioned in my researches on the phosphorus-bases. The idea naturally suggested itself to examine the deportment of this salt under the influence of monostibines, with the view of producing the phospho-stibonium-compounds. The two bodies react upon one another, but only after protracted digestion or exposure to rather high temperatures. The product of the reaction is complex, yielding a comparatively small quantity of a difficultly soluble platinum-salt of diatomic appearance. I have repeatedly modified the circumstances and analysed the products in the form of platinum-salts; I omit to quote the detail of these experiments, since they have failed to disentangle the difficulties of the reaction.

Some experiments upon the deportment of dibromide of ethylene with triethylarsine were more successful. The reaction between these two bodies had been selected as a subject of inquiry by Mr. W. Valentin, to whom I am indebted for valuable assistance at the earlier stage of these researches. Circumstances have subsequently prevented Mr. Valentin from carrying out his plan, and I have therefore to take upon myself the responsibility for the following statements.

Action of Dibromide of Ethylene upon Triethylarsine.

MONARSONIUM SERIES.

The experience gathered during the examination of the phosphorus-bodies, enabled me to establish the nature of this reaction by a comparatively small number of platinum-determinations.

Bromide of Bromethyl-triethylarsonium.—To avoid as far as possible the formation of the second product, a mixture of triethylarsine with a very large excess of dibromide of ethylene was digested in sealed tubes at a temperature not exceeding 50° C. Notwithstanding the low temperature, the tubes invariably contained compressed gases; the product of the reaction was treated with water, which extracted a soluble bromide from the ethylene-compound unacted upon. On evaporation, a beautiful bromide was left, which being copiously soluble in boiling, and sparingly soluble in cold alcohol, could be readily recrystallized from absolute, and even from common alcohol. In water this substance is excessively soluble, and therefore scarcely crystallizable from an aqueous solution.

Analysis, as might have been expected, proved this salt to be the analogue of the bromethylated triethylphosphonium-salt. It contains

$$C_{8}H_{19}AsBr_{2}=[(C_{2}H_{4}Br)(C_{2}H_{5})_{3}As]Br*.$$

The bromide of bromethyl-triethylarsonium, the composition of which is sufficiently established by the analysis of the corresponding platinum-salt, can be obtained in beautiful crystals. Their form was determined by Quintino Sella; it corresponds exactly with that of the corresponding phosphorus-compound.

Platinum-salt.—The solution of the previous salt, converted by treatment with chloride of silver into the corresponding chloride,

* H = 1; O = 16; S = 32; C = 12.

yields with dichloride of platinum, splendid needles of a double salt, difficultly soluble in cold and even in boiling water, which contain

$$C_8 H_{19} BrAs\,Pt\,Cl_3 = [(C_2 H_4 Br)\,(C_2 H_5)_3 As]\,Cl,\,Pt\,Cl_2.$$

Compounds of Vinyl-triethylarsonium.

The bromide of bromethyl-triethylarsonium, like the corresponding phosphorus-compound, loses its latent bromine under the influence of oxide of silver. If the solution of the bromide be precipitated by an excess of nitrate of silver, one half of the bromine separates as bromide of silver; the clear filtrate mixed with ammonia yields the second half of the bromine in the form of a dense precipitate. Nevertheless the reaction differs from that observed in the phosphorus-series. The bromide of the bromethylated phosphonium, as has been pointed out in a former part of the researches on the phosphorus-bases, is almost invariably converted into an oxethylated body, its transformation into a vinyl-compound being altogether exceptional. The bromide of the bromethylated arsonium, on the other hand, yields as a rule the vinyl-body of the series, the formation of an oxethylated compound taking place only under particular circumstances, in fact so rarely as to leave some doubt regarding the existence of this term of the series.

The bromide of bromethylated arsonium, treated with an excess of oxide of silver, yields a powerfully alkaline solution, the nature of which was determined by the analysis of the corresponding platinum-salt. Transformed into the chloride and precipitated with dichloride of platinum, this solution yielded beautiful rather soluble octahedra which were found to contain

$$C_6 H_{19} As\,Pt\,Cl_3 = [(C_2 H_3)\,(C_2 H_5)_3 As]\,Cl,\,Pt\,Cl_2.$$

The analysis of this salt shows that the transformation of the bromethylated compound ensues according to the following equation,

$$[(C_2 H_4 Br)\,(C_2 H_5)_3 As]\,Br + \left.\begin{matrix} Ag \\ Ag \end{matrix}\right\} O = \left[(C_2 H_3)\,(C_2 H_5)_3 \begin{matrix} As \\ H \end{matrix}\right] \Bigg\} O + 2\,Ag\,Br.$$

The idea suggested itself that the vinyl-compound obtained in this reaction might be a secondary product resulting from the decomposition of an oxethylated compound of limited stability formed in the first instance,

$$\left[(C_2 H_4 O)\,(C_2 H_5)_3 \begin{matrix} As \\ H \end{matrix}\right] \Bigg\} O = \left.\begin{matrix} H \\ H \end{matrix}\right\} O + \left[(C_2 H_3)\,(C_2 H_5)_3 \begin{matrix} As \\ H \end{matrix}\right] \Bigg\} O.$$

It was with the view of avoiding this decomposition that in one of the operations the digestion was accomplished at the common temperature; the result, however, showed that even in this case the vinyl-compound was obtained.

Nevertheless the oxethylated body appears to exist: under circumstances which were not sufficiently well observed at the time, the action of oxide of silver upon bromide of bromethylated triethylarsonium yielded an octahedral platinum-salt, which on analysis furnished exactly the platinum-percentage of the oxethylated compound.

Diarsonium Series.

Dibromide of Ethylene-hexethyldiarsonium.

The bromide or chloride of the bromethylated arsonium-compound is but slowly acted upon by triethylarsine at 100° C. Two days' digestion at that temperature had produced but a slight impression ; at 150° the reaction is accomplished in two hours. The phenomena now to be recorded presented themselves in the succession repeatedly observed in the diphosphonium-series. The dibromide

$$C_{14}H_{34}As_2Br_2=(C_2H_4)''\begin{matrix}(C_2H_5)_3As\\(C_2H_5)_3As\end{matrix}\Big]''Br_2$$

yielded, when debromized, the powerful alkali

$$C_{14}H_{36}As_2O_2=\left.\begin{matrix}[(C_2H_4)''\ (C_2H_5)_6As_2]''\\H_2\end{matrix}\right\}O_2.$$

Treated with acids, this alkali produces a series of fine salts, amongst which the di-iodide deserves to be mentioned ; it equals in beauty the corresponding diphosphonium-compound.

I have fixed the composition of the series by the analysis of the platinum-salt and gold-salt.

Platinum-salt.—Pale-yellow crystalline precipitate, similar to the diphosphonium-compound, difficultly soluble in water, soluble in boiling concentrated hydrochloric acid, from which it crystallizes on cooling. It contains

$$C_{14}H_{34}As_2Pt_2Cl_6=\left[(C_2H_4)''\begin{matrix}(C_2H_5)_3As\\(C_2H_5)_3As\end{matrix}\right]''Cl_2,2PtCl_2.$$

Gold-salt.—The dichloride obtained after separating the platinum in the previous analysis by sulphuretted hydrogen, was precipitated by trichloride of gold ; yellow slightly crystalline precipitate, soluble in hydrochloric acid, from which it crystallizes in golden-coloured plates. The formula of this salt is

$$C_{14}H_{34}As_2Au_2Cl_6=\left[(C_2H_4)''\begin{matrix}(C_2H_5)_3As\\(C_2H_5)_3As\end{matrix}\right]''Cl_2,2AuCl_3.$$

Arsammonium Series.

Bromide of bromethylated triethylarsonium, as might have been expected, is capable of fixing ammonia and monamines, giving rise to the formation of a group of compounds not less numerous than the bodies mentioned in the phosphorus-series. I have been satisfied to study the action of ammonia upon the bromide.

Dibromide of Ethylene-triethylarsammonium.

Reaction complete in two hours at 100°. The product contains the dibromide,

$$C_8H_{22}AsNBr_2=\left[(C_2H_4)''\begin{matrix}(C_2H_5)_3As\\H_3N\end{matrix}\right]Br_2;$$

this salt is converted by oxide of silver into the stable caustic base

$$C_8H_{24}AsNO_2=\left.\begin{matrix}[(C_2H_4)''\ (C_2H_5)_3H_3AsN]''\\H_2\end{matrix}\right\}O_2,$$

the composition of which was determined by the analysis of the platinum-salt and gold-salt.

Platinum-salt. — Needles, difficultly soluble in boiling water, soluble in concentrated hydrochloric acid, from which well-formed crystals are deposited, containing

$$C_8 H_{22} As N Pt_2 Cl_6 = \left[(C_2 H_4)'' \begin{matrix}(C_2 H_5)_3 As \\ H_3 N\end{matrix}\right]'' Cl_2, 2 Pt Cl_2.$$

Gold-salt.—Yellow compound precipitated from the dichloride obtained in the previous platinum-determination, on addition of trichloride of gold, soluble in hydrochloric acid, deposited from this solution in golden-yellow plates of the composition

$$C_8 H_{22} As N Au_2 Cl_8 = [(C_2 H_4)'' (C_2 H_5)_3 H_3 As N]'' Cl_2, 2 Au Cl_3.$$

I have also made a few experiments on the action of dibromide of ethylene upon triethylstibine. The reaction is slow, and requires long-continued digestion at temperatures higher than that of boiling water. The tubes invariably contained much gas; and the product of the reaction proved to be a complex mixture of several compounds, many of them secondary, which in no way invited me to a more minute examination of this process. I omit to quote the few platinum- and chlorine-determinations which were made, since they do not admit of a simple interpretation.

"Contributions towards the History of the Monamines."—No. IV. Separation of the Ethyle-Bases. By A. W. Hofmann, LL.D., F.R.S.

The preparation of the ethyle-bases by the action of ammonia upon iodide of ethyle, presents a difficulty which greatly interferes with the general application of this otherwise so convenient method. This difficulty consists in the simultaneous formation of all the four ethyle-bases. The equations

$$\begin{aligned}
H_3 N + C_2 H_5 I &= [(C_2 H_5)\ H_3 N] I^* \\
(C_2 H_5)\ H_2 N + C_2 H_5 I &= [(C_2 H_5)_2\ H_2 N] I \\
(C_2 H_5)_2\ H\ N + C_2 H_5 I &= [(C_2 H_5)_3\ H\ N] I \\
(C_2 H_5)_3\ \ N + C_2 H_5 I &= [(C_2 H_5)_4\ \ N] I,
\end{aligned}$$

are an ideal representation of the four different phases through which ammonia passes during its transformation into iodide of tetrethylammonium. In practice it is found impossible to carry out this transformation in the several steps indicated by these equations. The first substitution-product, generated as it is in the presence of the agent of substitution, is immediately acted upon again, the second product being formed, which in its turn may be converted into the third and even into the fourth compound. The following equations represent perhaps more correctly the final result of the several changes which are accomplished in the reaction of ammonia on iodide of ethyle.

$$\begin{aligned}
H_3 N + \ C_2 H_5 I &= [(C_2 H_5)\ H_3 N] I \\
2\,H_3 N + 2\,C_2 H_5 I &= [(C_2 H_5)_2\ H_2 N] I + \ [H_4 N] I \\
3\,H_3 N + 3\,C_2 H_5 I &= [(C_2 H_5)_3\ H\ N] I + 2\,([H_4 N] I). \\
4\,H_3 N + 4\,C_2 H_5 I &= [(C_2 H_5)_4\ \ N] I + 3\,([H_4 N] I).
\end{aligned}$$

* H=1; O=16; C=12, &c.

The mixture of iodides, when submitted to the action of potassa, yields ammonia, ethylamine, diethylamine, and triethylamine, the hydrate of tetrethylammonium, which is liberated, splitting into ethylene, triethylamine, and water. The separation of the three ethyle-ammonias presents unusual difficulties. The differences between their boiling-points being rather considerable,

Ethylamine, boiling-point	18°
Diethylamine, „ „	57°·5
Triethylamine, „ „	91°

it was thought that they might be readily separated by distillation. Experiments made with very large quantities showed, however, that even after ten fractional distillations the bases were far from being pure.

After many unsuccessful attempts, I have found a simple and elegant process by which the three ethyle-bases may be easily and perfectly separated. This process consists in submitting the anhydrous mixture of the three bases to the action of anhydrous oxalate of ethyle. By this treatment, ethylamine is converted into *diethyl-oxamide*, a beautifully crystalline body very difficultly soluble in water, diethylamine into *ethyl-oxamate of ethyle*, a liquid boiling at a very high temperature, whilst triethylamine is not affected by oxalic ether

By the action of oxalic ether upon ethylamine, two substances may be formed, viz. *ethyl-oxamate of ethyle* and *diethyl-oxamide*.

$$\left.\begin{matrix}(C_2O_2)''\\(C_2H_5)_2\end{matrix}\right\}O_2+\left.\begin{matrix}C_2H_5\\H\\H\end{matrix}\right\}N=\left.\begin{matrix}[(C_2O_2)''(C_2H_5)HN]\\(C_2H_5)\end{matrix}\right\}O+\left.\begin{matrix}C_2H_5\\H\end{matrix}\right\}$$

Oxalic Ether. Ethylamine. Ethyl-oxamate of ethyle. Alcohol.

$$\left.\begin{matrix}(C_2O_2)''\\(C_2H_5)_2\end{matrix}\right\}O_2+2\left[\left.\begin{matrix}C_2H_5\\H\\H\end{matrix}\right\}N\right]=\left.\begin{matrix}(C_2O_2)''\\(C_2H_5)_2\\H_2\end{matrix}\right\}N_2+2\left[\left.\begin{matrix}C_2H_5\\H\end{matrix}\right\}O\right].$$

Oxalic Ether. Ethylamine. Diethyl-oxamide. Alcohol.

In practice it appears that the *second* of these compounds only is produced.

In the action of oxalate of ethyle upon diethylamine, two similar phases may be distinguished capable of producing respectively

Diethyl-oxamate of ethyle .... $\left.\begin{matrix}[(C_2O_2)''(C_2H_5)_2N]\\C_2H_5\end{matrix}\right\}O$, and

Tetrethyl-oxamide ..... $\left.\begin{matrix}(C_2O_2)''\\(C_2H_5)_2\\(C_2H_5)_2\end{matrix}\right\}N_2$.

In practice the *first* of these two compounds only is generated.

The action of oxalate of ethyle upon triethylamine might have involved the formation of the secondary oxalate of tetrethylammonium,

$$\left.\begin{matrix}(C_2O_2)''\\(C_2H_5)_2\end{matrix}\right\}O_2+2\left[\left.\begin{matrix}C_2H_5\\C_2H_5\\C_2H_5\end{matrix}\right\}N\right]=\left.\begin{matrix}(C_2O_2)''\\[(C_2H_5)_2N]_2\end{matrix}\right\}O_2;$$

under the circumstances under which I have worked, the two substances do not combine.

The product of the reaction of oxalate of ethyle upon the mixture of the ethyle-bases, when distilled in the water-bath, yields *triethylamine free from ethylamine and diethylamine.*

The residue in the retort solidifies on cooling into a fibrous mass of crystals of diethyloxamide, which are soaked with an oily liquid. They are drained from the oil and recrystallized from boiling water. Distilled with potassa, these crystals furnish *ethylamine free from diethylamine and triethylamine.*

The oily liquid is cooled to 0°, when a few more of the crystals are deposited; it is then submitted to distillation. The boiling-point rapidly rises to 260°. What distils at that temperature is pure diethyl-oxamate of ethyle, from which, by distillation with potassa, *diethylamine free from ethylamine and triethylamine* may be obtained.

January 10, 1861.—Major-General Sabine, R.A., Treasurer and Vice-President, in the Chair.

The following communication was read:—

"On the Lunar-diurnal Variation of the Magnetic Declination obtained from the Kew Photograms* in the years 1858, 1859, and 1860." By Major-General Edward Sabine, R.A., Treas and V.P.R.S.†

Having communicated to the Royal Society in a recent paper an analysis of the *disturbances* of the declination in the years 1858 and 1859, shown by the photograms of the Kew Observatory, I propose in the present paper to submit the results of the *lunar-diurnal variation* of the declination in the years 1858, 1859, and 1860, obtained from the same source. The directions of the declination magnet at the instant of the commencement of every solar-hour having been tabulated from the photograms, and the final normals for each month and hour computed, after the omission from the record of all the hourly directions which deviated 3′·3 from their final normals,—the *differences* were taken between each of the remaining hourly directions and the final normal of the same month and hour, and were entered afresh in *lunar* monthly tables, having the lunar days in successive horizontal lines, and the twenty-four lunar hours in vertical columns, each difference being placed under the lunar hour to which it most nearly approximated. The entries in these tables should consequently represent directly the lunar influence at the different lunar hours, subject only to minor disturb-

* The term Photogram is adopted in place of Photograph in conformity with modern usage.

† [Note added on February 8th, 1861.] When this communication was read to the Royal Society on January 10th, 1861, it contained the lunar-diurnal variation for the years 1858 and 1859 only; whilst it was passing through the press, the calculation of the lunar-diurnal variation for 1860 was completed, and the results in that year have been added.

ances; the effects of the solar-diurnal variation as well as of the larger disturbances having been eliminated. The differences were marked with a + sign when the north end of the magnet was east of its mean direction, and with the − sign when west of the same. The differences were then summed up, and hourly, monthly, and annual means taken by the non-commissioned officers of the Royal Artillery employed at Woolwich, under the superintendence of Mr. Magrath.

Having in the former paper exhibited the results of the disturbances at Kew in comparison with those at Hobarton, I propose to do the same with the lunar-diurnal variation treated of in this communication; believing that such comparisons are very conducive to a just appreciation of the systematic character and natural reality of the results, and instructive both by the agreements and disagreements which they exhibit. The lunar-diurnal variation at Hobarton has been obtained for the purpose of this comparison, by a similar process to that which has been described above, from observations at every solar hour during five years (Sundays excepted), from Oct. 1, 1843 to Sept. 30, 1848; omitting as disturbed such observations as deviated 2′·13 from their respective final normals. The total number of hourly observations was 36,832; the disturbed observations 2606; and the number employed in the lunar-diurnal variation 34,226. As it has been customary to represent such periodical variations by formulæ of well-known character, the results at Kew and Hobarton are here represented by formulæ in which a, corresponding to x (the lunar time for which the lunar-diurnal variation is desired), is counted in hours and parts of an hour, multiplied by 15°, from the epoch of the moon's upper culmination. The + sign corresponds (as before) to a deflection of the north end of the magnet to the east of its mean place, and the − sign to the west.

Kew $\Delta x = +0''\cdot64 - 2''\cdot54 \sin(a + 6°\cdot2) - 9''\cdot74 \sin(2a + 59°\cdot8)$.

Hobarton $\Delta x = -0''\cdot1 + 1''\cdot14 \sin(a + 344°\cdot7) + 6''\cdot8 \sin(2a + 43°\cdot2)$.

In computing the lunar-diurnal variation by means of these formulæ, the coefficient of the term which includes the sine of twice the hour-angle is of principal importance: the subsequent terms are comparatively of little significance, and are therefore omitted on the present occasion. When all the terms are employed, the original observed values are reproduced.

Table I. exhibits, at Kew, in column 2 the lunar-diurnal variation as actually observed on the mean of the three years, and in column 3, the same computed by the formula. Column 4 is the lunar-diurnal variation at Hobarton on the mean of the five years as observed, and column 5 the same computed by the formula.

TABLE I.—Lunar-diurnal Variation at Kew and Hobarton.

Lunar Hours.	Kew.		Hobarton.		Lunar Hours.
	Observed.	Computed.	Observed.	Computed.	
Col. 1.	Col. 2.	Col. 3.	Col. 4.	Col. 5.	Col. 6.
0	− 6·0	− 8·0	+ 4·8	+ 4·3	0
1	−11·4	−10·0	+ 6·1	+ 6·4	1
2	− 8·6	− 9·3	+ 5·2	+ 6·8	2
3	− 5·0	− 6·2	+ 5·9	+ 5·4	3
4	− 3·2	− 1·7	+ 4·2	+ 2·7	4
5	+ 1·4	+ 3·0	0·0	− 0·6	5
6	+ 5·4	+ 6·5	− 4·9	− 3·7	6
7	+ 7·6	+ 8·0	− 6·1	− 5·5	7
8	+ 8·6	+ 7·0	− 4·9	− 5·6	8
9	+ 4·3	+ 3·9	− 3·3	− 4·1	9
10	+ 2·8	− 0·4	− 3·2	− 4·3	10
11	− 3·0	− 4·6	+ 3·6	+ 2·0	11
12	−10·6	− 7·5	+ 4·9	+ 4·9	12
13	−10·4	− 8·2	+ 6·6	+ 6·4	13
14	− 7·0	− 6·3	+ 5·9	+ 6·2	14
15	− 2·2	− 2·3	+ 4·1	+ 4·3	15
16	+ 4·8	+ 3·0	+ 1·4	+ 1·1	16
17	+10·4	+ 8·0	− 3·4	− 2·7	17
18	+13·2	+11·6	− 6·4	− 5·9	18
19	+12·6	+12·7	− 6·5	− 7·7	19
20	+ 7·2	+11·1	− 6·6	− 7·8	20
21	+ 6·2	+ 7·1	− 8·4	− 6·0	21
22	− 0·4	+ 1·7	− 1·9	− 2·9	22
23	− 1·4	− 4·6	+ 0·8	+ 0·9	23

The aspect of the lunar-diurnal variation at Kew and Hobarton presents features of great simplicity as well as accord. The form at both stations is a division of the 24 lunar hours into four equal or nearly equal portions, in which the magnet is attracted alternately to the east and to the west of its mean position, which is passed through four times in the progress of the magnet towards two extreme easterly and two extreme westerly deflections: the easterly extremes are about 12 hours apart, and the westerly the same. As far as our present experience goes, this appears to be the general form of the lunar-diurnal variation of the declination at all the stations at which it has been examined; it is also that of the corresponding variations of the Dip and Total force. At Hobarton, where the results are obtained from five years of observation, there is scarcely any difference deserving of notice between the amplitudes of the extremes on either side of the upper culmination and those on either side of the lower culmination. At Kew, where the results are obtained from only three years, the extreme deflections are not quite so symmetrical in amount, but they may become more so as additional years are brought into the account. The amplitude of the oscillation on a mean of the two alternations is 9″·74 at Kew and 6″·8 at Hobarton, a difference in correspondence with the difference in the opposite direction of the antagonistic retaining force of the earth's magnetism at the two stations, which is 3·7 at Kew and 4·5 at Hobarton. On inspecting

the Table, we see that the lunar times when the moon's influence produces no deflection (or the times when the variation is zero), are four, and are nearly the same at Kew and at Hobarton, two of them being a little more than an hour before the moon's passage of the meridian, both at her upper and lower culminations, and the other two intermediate. So far the two stations are alike; but in regard to the direction towards which the magnet is deflected (if in conformity with general usage we speak in both hemispheres of the north end of the magnet, as is done in the Table), we see that the variation becomes west at Kew when it becomes east at Hobarton, and *vice versâ*; the phases, while agreeing in hours at the two stations, having throughout opposite signs.

By extending the comparison of the lunar hours at which the lunar variation passes through its zero-points to other stations than Kew and Hobarton, we are made aware of differences which appear to deserve particular attention in theoretical respects. At Pekin, for example—which may be advantageously compared with Kew, being both in the same hemisphere, but Pekin some degrees nearer the equator—the variation is zero in the passage of the north end of the magnet from east to west at $20\frac{1}{2}$ lunar hours, or $2\frac{1}{2}$ hours earlier than the corresponding epoch at Kew. Again, at the Cape of Good Hope, situated in the same hemisphere with Hobarton, but some degrees nearer the equator, the variation is zero in the passage of the north end of the magnet from west to east also at $20\frac{1}{2}$ lunar hours, or $2\frac{1}{2}$ hours earlier than the corresponding epoch at Hobarton. Thus there is an accord of precisely the same kind between Pekin and the Cape of Good Hope that there is between Kew and Hobarton, whilst there is a difference between the two pairs of stations of $2\frac{1}{2}$ hours in the position of the moon relatively to the meridian at which she ceases to exercise a deflecting influence on the magnet. Again, at St. Helena, which is in the same (geographical) hemisphere as Hobarton and the Cape of Good Hope, but still nearer to the equator than either, the lunar influence is zero in the passage from west to east at $19\frac{1}{2}$ lunar hours, being one hour earlier than at the Cape, and $3\frac{1}{2}$ hours earlier than at Hobarton.

Where the whole range of the variation of which we have been treating is so small (not more than a few seconds of arc in each lunar day), it may be desirable to show by the accordance of the independent evidence obtained in single years, the degree of confidence which may be placed in the mean results of several years. This may be seen in the Table on the next page, which contains the separate results in each of the five successive years of observation at Hobarton, as well as their mean.

In this Table the principal features of the variation are seen to be substantially alike in each year. The individual results at the several hours in single years are of course somewhat less regular than in the mean of the five years: such small discrepancies are no doubt in great part due to the lesser disturbances which, being below the separating value of 2″·13, have been left in the body of the observations. They slightly disfigure the symmetry of the results in single years, but almost entirely disappear when the mean of several years is taken. In

order to appreciate justly and fully the confidence to which the whole investigation is entitled, it must be borne in mind that every single entry in the Table (exclusive of course of the column which exhibits the mean of the five preceding columns) is derived from a wholly independent body of observations which belong to itself alone, and are not employed in the deduction of any of the other entries.

TABLE II. — Lunar-diurnal Variation at Hobarton in the several years from October 1843 to September 1848; omitting disturbed observations differing 2′·13 from their final normals.

Lunar Hours.	Years ending September 30th.					Means.	Lunar Hours.
	1844.	1845.	1846.	1847.	1848.		
	″	″	″	″	″	″	
0	+ 0·6	+ 7·8	+ 3·6	+ 2·4	+ 9·6	+ 4·8	0
1	+ 6·6	+ 9·0	+ 1·2	+ 0·6	+13·2	+ 6·1	1
2	+ 4·8	+ 5·4	+ 5·4	+ 6·0	+ 4·2	+ 5·2	2
3	+ 9·6	+ 7·8	+ 7·8	+ 3·6	+ 0·6	+ 5·9	3
4	+ 4·8	+ 6·6	+ 6·0	+ 3·0	+ 0·6	+ 4·2	4
5	− 3·0	+ 2·4	+ 3·0	− 1·8	− 0·6	0·0	5
6	− 7·8	− 6·0	− 1·8	− 7·8	− 1·2	− 4·9	6
7	− 6·0	− 9·6	− 0·6	−10·8	− 3·6	− 6·1	7
8	− 4·2	− 8·4	− 1·2	− 7·8	− 3·0	− 4·9	8
9	0·0	− 9·0	− 0·6	− 4·8	− 3·0	− 3·3	9
10	− 2·4	− 4·8	− 1·8	− 0·6	− 6·6	− 3·2	10
11	+ 3·0	+ 0·6	+ 3·0	+ 8·4	+ 3·0	+ 3·6	11
12	+ 7·2	+ 2·4	+ 4·8	+ 7·8	+ 2·4	+ 4·9	12
13	+12·0	+ 6·6	+ 6·6	+ 4·8	+ 3·0	+ 6·6	13
14	+ 3·0	+ 8·4	+ 7·2	+ 7·8	+ 3·0	+ 5·9	14
15	+ 7·8	+ 4·8	+ 4·2	+ 3·0	+ 0·6	+ 4·1	15
16	+ 1·8	+ 3·6	+ 0·6	+ 0·6	+ 0·6	+ 1·4	16
17	0·0	− 1·2	− 3·6	− 2·4	− 9·6	− 3·4	17
18	− 6·6	− 6·6	− 5·4	− 6·6	− 6·6	− 6·4	18
19	− 4·8	− 5·4	− 8·4	− 7·2	− 6·6	− 6·5	19
20	− 6·6	−10·2	−13·2	− 4·2	− 5·4	− 6·6	20
21	− 9·6	−12·6	−10·8	− 4·2	− 4·8	− 8·4	21
22	− 4·2	− 0·6	−10·8	+ 2·4	+ 3·6	− 1·9	22
23	− 2·4	0·0	− 2·4	+ 0·6	+ 8·4	+ 0·8	23

It may operate as an encouragement to those who have not yet subjected their observations to any process of examination or analysis, to perceive, by this example, how substantially satisfactory are the results which may be obtained from even a single year of hourly observations, after the larger disturbances and the solar-diurnal variation have been eliminated.

I have spoken in a recent paper of an unexceptionable test by which we may satisfy ourselves as to the confidence which may be reposed in a series of observations, whether obtained by the eye or tabulated from instrumental traces. Such a test is furnished when the entries at solar hours are rewritten according to the lunar hours to which they most nearly approximate, and when consequently their original order and relations are changed and are replaced by others which were wholly unforeseen, so that the observations must necessarily be free from the possibility of having been influenced by any mental bias. When we find the effects of a natural law, represented by such minute values as that of the lunar-diurnal variation, exhibited by the

observations of a single year with the degree of symmetry shown in Table II., we may safely conclude that the observations themselves are worthy of the labour bestowed in eliciting their results. In this view the Hobarton observations prove themselves to have been not only a faithful, but also an extremely careful series, highly creditable to Captain Kay, R.N., and to the Naval Officers who with him and their Civil Assistant Mr. Jeffery, maintained for so many years the laborious and monotonous duty of hourly observation.

Table III. exhibits the separate results in each of the three years at Kew, as well as their mean.

TABLE III.—Lunar-diurnal Variation at Kew in the years 1858, 1859, and 1860; omitting disturbed observations differing 3″·3 from their final normals.

Lunar Hours.	Year ending December 31.			Means.	Lunar Hours.
	1858.	1859.	1860.		
	″	″	″	″	
0	− 6·0	+ 0·6	−12·6	− 6·0	0
1	−14·4	− 7·2	−12·6	−11·4	1
2	−10·8	− 9·6	− 5·4	− 8·6	2
3	− 7·8	− 4·2	− 3·0	− 5·0	3
4	− 3·0	− 4·2	− 2·4	− 3·2	4
5	+ 5·4	− 6·6	+ 5·4	+ 1·4	5
6	+ 2·0	+ 1·2	+ 3·0	+ 5·4	6
7	+ 9·0	+ 4·2	+ 9·6	+ 7·6	7
8	+19·6	+ 8·4	+ 7·8	+ 8·6	8
9	+ 7·2	+ 6·6	− 0·9	+ 4·3	9
10	+ 3·0	+ 7·2	− 1·8	+ 2·8	10
11	− 3·6	− 1·2	− 4·2	− 3·0	11
12	− 4·8	− 9·0	−18·0	−10·6	12
13	− 3·0	−13·2	−15·0	−10·4	13
14	− 3·0	− 8·4	− 9·6	− 7·0	14
15	− 7·2	− 3·6	+ 4·2	− 2·2	15
16	+ 3·0	+ 3·6	+ 7·8	+ 4·8	16
17	+ 7·8	+ 9·6	+13·8	+10·4	17
18	+ 7·8	+14·4	+17·4	+13·2	18
19	+ 4·8	+18·0	+15·0	+12·6	19
20	+ 3·0	+12·6	+ 6·0	+ 7·2	20
21	− 2·4	+18·6	+ 2·4	+ 6·2	21
22	− 7·8	+ 9·6	− 3·0	− 0·4	22
23	− 6·0	+ 5·4	− 3·6	− 1·4	23

In conclusion, it may be useful to call the attention of the Society, and of those Fellows in particular who interest themselves in tracing up the phenomena of nature to their physical causes, to the assemblage of facts which are now available for such inquiries, in a branch of magnetical science which may not inappropriately be called *celestial magnetism*. In the introductory discussion prefixed to the 2nd volume of the St. Helena Magnetical Observations, p. cxliv to cxlviii, the lunar-diurnal variation is given for each of the three magnetic elements, the Declination, the Dip, and the Intensity of the force, at the four stations of Toronto, St. Helena, the Cape of Good Hope and Hobarton, and for the Declination at two additional stations Kew and Pekin. The variations are given both in formulæ and in

tables; the latter exhibiting the amount of the lunar influence at each of the 24 lunar hours, in the several magnetic elements at each station. These data are directly applicable to inquiries into the nature of the moon's magnetism; and into the mode by which the moon's magnetism acts either on the magnetism of the earth itself, or on the magnetic needle stationed at different points of the earth's surface, so as to produce a small but systematic and perfectly appreciable variation in each of the magnetic elements, having a double period in every lunar day.

The lunar-diurnal variation of the Declination at Kew and Hobarton, as given in this communication, is slightly different from the figures in the 2nd St. Helena volume referred to, because the results at Kew are a mean of 3 years instead of 2, as in the St. Helena volume; and at Hobarton a lower standard has been taken for the disturbances, causing a larger number of the disturbed observations to be omitted in the calculation of the lunar-diurnal variation.

January 24.—Major-General Sabine, Treas. and V.P., in the Chair.

The following communications were read:—

"On the Calculus of Symbols, with Applications to the Theory of Differential Equations." By W. H. A. Russell, A.B.

"On the Properties of Liquid Carbonic Acid." By G. Gore, Esq.

In this communication the author has shown how a small quantity of liquid carbonic acid may be readily and safely prepared in glass tubes closed by stoppers of gutta percha, and be brought in a pure state into contact with any solid substance upon which it may be desired to ascertain its chemical or solvent action, or be submitted to the action of electricity by means of wires introduced through the stoppers.

By immersing about fifty substances in the liquid acid for various periods of time, he has found that it is comparatively a chemically inert substance, and not deoxidized by any ordinary deoxidizing agent except the alkali-metals. Its solvent power is extremely limited; it dissolves camphor freely, iodine sparingly, and a few other bodies in small quantities; it does not dissolve oxygen-salts, and it does not redden solid extract of litmus; it penetrates gutta percha, dissolves out the dark-brown colouring matter, and leaves the gutta percha undissolved, and much more white. It also acts in a singular and somewhat similar manner upon india-rubber; the india-rubber whilst in the liquid acid exhibits no change, but immediately on being taken out it swells to at least six or eight times its original dimensions, and then slowly contracts to its original volume, evidently from expansion and liberation of absorbed carbonic acid; and it is found to be perfectly white throughout its substance. These effects upon gutta percha and india-rubber may prove useful for practical purposes.

The liquid acid is a strong insulator of electricity; sparks (from a Ruhmkorff's coil) which would pass readily through $\frac{9}{10}$nds of an inch of cold air, would with difficulty pass through about $\frac{1}{70}$th of an inch of the liquid acid.

In its general properties it is somewhat analogous to bisulphide of carbon, but it possesses much less solvent power over fatty substances,

LXII. *Intelligence and Miscellaneous Articles.*

LUNAR RADIATION.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN, Oxford and Cambridge Club, Nov. 19, 1861.

IN your November Number there is an account by Professor Tyndall of some observations with a thermo-electric pile, in the course of which it appeared that it "lost more heat when presented to the moon than when turned to any other portion of the heavens of the same altitude;" and there is a theoretical explanation of this fact as an indirect effect of the moon's heat, dispersing the "small quantity of precipitated vapour" which it appears was then floating in the atmosphere, and so facilitating radiation from the instrument.

Unless my memory is deceived, Sir John Herschel, in one of the earlier editions of his 'Astronomy,' described light clouds as, in like manner, dispersing *as they came between his telescope and the moon*; but in the edition of 1858, here at hand, I see the phrase is "the tendency to disappearance of clouds *under the full moon*," which may mean a very different thing, viz. a tendency to *clear skies when the moon is full*.

That the heat of the full moon may tend to clear the upper atmosphere, and so be the cause of cold below, may be true; but it does not appear to me that this can be the explanation of Professor Tyndall's fact, or of Sir J. Herschel's, if I state it correctly.

High in the air, in the region in which the moon is seen, there is cloud or vapour observed. The moon may have diminished, but it has not destroyed it *generally*. How then is that particular portion which happens to intervene between the observer's instrument and the moon more under her influence than any other equal portion? If a hundred observers were gazing at her at the same time within a few miles of each other, a hundred different portions of the haze would so intervene; and to suppose each of these dispersed, is to suppose the haze not to exist.

It is possible that a full examination of all the circumstances of Professor Tyndall's six experiments—the area embraced by his reflector, the probable height of the vapour in the air, the extent of the sweep he took with the instrument, &c.—might remove some of the difficulty I feel in admitting the explanation he proposes; and in the interest of exact science I venture to call his attention to the matter.

D. D. HEATH.

ON THE DIHEXAHEDRAL CRYSTALS OF SULPHATE OF POTASH. BY KARL RITTER VON HAUER*.

The supposed dimorphism of the sulphate of potash, as K. von Hauer has proved, rests only on external appearance, as in reality this salt in a state of chemical purity constantly affects forms of the prismatic system, and when appearing in forms of the rhombohedral system invariably contains a certain quantity of anhydrous sulphate of soda. This bibasic salt is known to be produced at Glasgow, in the shape of hexagonal plate-like crystals, by the evaporation of a solution of kelp-ash. A mixture of both these sulphates (potash and soda), inspissated and left to crystallize, invariably gives no longer hexagonal plates, but exclusively dihexahedrons (double hexagonal

* Translated by Count Marschall.

pyramids), a form scarcely if ever met with among the crystals produced by the above-described technical process. Analogous local actions are observed on natural minerals; so that in some cases an expert mineralogist may infer the place of origin of a mineral substance from its crystalline form only. On the other hand, conclusions as to the mode of formation of minerals founded on the results of laboratory experiments must be drawn with a certain degree of caution. In fact, the chemical forces, when acting on large quantities of substances, as in manufacturing processes, frequently produce results very different from those obtained by the chemist operating with comparatively small portions; and, still more, the results of natural operations, gigantic in quantity as in energy, and extending through immeasurable periods of time, may scarcely be comparable to mere laboratory investigations made with limited quantities in some few hours or days.

When immersed in solutions of other salts, the crystals of the bibasic sulphate in question show some curious phenomena. In a solution of sulphate of ammonia a hexagonal plate was gradually converted, by superposition on both of its larger planes, into a lengthened hexagonal prism, easily cleavable at any point in a direction perpendicular to its longitudinal axis. Thin plates of it taken from the newly added portion show the characteristic optical properties of the common prismatic sulphate of ammonia. This instance of episomorphism between a rhombohedral and a prismatic salt, or, in other words, of two substances belonging each to a different crystallographical system and nevertheless subject to the crystallographical laws of isomorphism, is highly interesting. The angular values of both (the rhombohedral and the prismatic combination) being, in this special case, very near each other, the existence of the fact here alluded to was to be decided by optical investigation. Trifling as the difference of the forms here in question may be, its existence is a fact not to be denied; and therefore such a formation as just described could not take place if the disposition of the molecules, by whose regular aggregation such crystals are formed, did not go on with mathematical exactitude. Observation shows deviations from the strict regularity of lines and angles to be of no rare occurrence in crystallogenetic processes; precise measurements of substances considered to be isomorphous have shown them not to be absolutely congruent; so that isomorphism, as far as it is concerned in this character, has only an approximate value. Two substances different in angular value, even when combined into one and the same crystal, cannot be considered as having totally lost their respective individuality. Their last constituent parts, representing the crystalline molecules of both salts (sulphates of potash and ammonia), are in juxtaposition to each other, as if they were but one homogeneous substance. Their superposition without preceding mixture is a proof that molecules of not absolute identity may be deposited on each other in the same way as analogous particles would be. Both these sulphates could be considered as absolutely isomorphous in the crystallographical sense, but for the optical phenomena characteristic of two distinct and mutually independent systems. Isomorphism, however, presupposes chemical analogy; now the potash

being very prevalent in quantity in the rhombohedral bibasic salt, its chemical analogy with ammonia may be supposed to have been superseded by the comparatively small proportion of soda combined with it. In the absence of isomorphism in the strictest sense, there are circumstances coming so near to it that the molecules of both substances still attract each other sufficiently to effect regular superposition. The curved and disfigured planes of such crystals are at all events indicative of their origin under abnormal and, as it were, compulsory circumstances.—*Proceedings of the Vienna Imperial Institute*, April 16, 1861.

COMPARISON OF THE TEMPERATURE IN THE AIR AND OF THE SOIL AT A DEPTH OF TWO METRES. BY M. POURIAU.

From observations made during five consecutive years on the temperature of the soil at a depth of 2 metres compared with that of the air, it follows—

1. That the mean temperature in the air was 10°·21, and in the soil 12°·79. Difference in favour of the soil 2·58.

2. That the mean temperature of the soil in winter and autumn is higher than that of the air; that in summer it is about 2 degrees lower, and that in spring the mean temperatures are virtually equal.

3. That the mean of the extreme maximum temperatures in the air was 34°·5, in the soil it was 19°·75. On the other hand, the mean of the extreme minima in air was −12°·14; in the soil this mean never sank below +6°.

4. While in air the mean of the total differences between the extreme maxima and extreme minima reached 46°·64, in the soil this mean was only 13°·74.

5. In 1860 the temperature of the air sank to −20°, in the soil the minimum was never less than +5°·47.

6. While in the air the maximum temperature usually occurs in July or August, and the minimum in December or January, the maximum temperature in the soil always corresponds to the end of August; the minimum always occurs at the end of February, or on the first days of March.

7. The changes of temperature in the soil at a depth of 2 metres may be thus stated:—

While the mean temperature of the air usually begins to sink towards the end of July, in the soil the heat continues to accumulate in the superior layers under the influence of the intense solar radiation, and to extend to the lower layers, until the end of August. From this point the upper layers begin to lose more heat by radiation than they receive; the flow of heat changes its direction, it passes from the lower to the upper layers and becomes lost in the air; and this ascending motion, continuing until February, is more rapid as the external temperature is lower, that is, as the winter is longer and more severe. Towards the middle of February or the beginning of March the upper layers begin to become heated under the influence of the solar rays, whose direction has become less oblique; the inferior layers give less and less heat to the upper ones; they begin, on the contrary, to receive some, and become then reheated, which continues until the end of August.—*Comptes Rendus*, October 7, 1861.

THE

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SUPPLEMENT TO VOL. XXII. FOURTH SERIES.

LXIII. *Static and Dynamic Stability in the Secondary Systems.* *By* DANIEL VAUGHAN, *Esq.**

SO small are the primary planets compared with their distances from the sun, that they are regarded as material points in the investigations of physical astronomy, and that no effects arising from the unequal intensity of solar attraction on their parts can vitiate in any sensible degree the results which analysis gives for their movements. But in the systems of Jupiter and Saturn many satellites are exposed to an enormous tidal force in consequence of their proximity to their primaries; and the planetary theory requires some modification when applied to the revolutions of these minor worlds. My chief object at present is to show that the unequal attraction of a primary occasions slow secular changes in the orbits of its attendants, especially when the presence of fluids on their surfaces brings tidal commotions into play. But I deem it first necessary to prove what I have assumed in my former communications, in regard to the physical necessity for a synchronism of the orbital and rotatory motions of these bodies, and for the small inclination of their equators to the planes of their orbits.

On previous occasions I endeavoured to show that such an arrangement would be the ultimate consequence of excessive tides, when a satellite contained large bodies of fluid, or when, from its close proximity to the primary, the solid matter of which it may be composed were not possessed of sufficient cohesive force to withstand the effects of the great disturbance. But the same result would ultimately arise from slow secular changes which must occur in every possible case. Let us suppose, for instance, that the first satellite of Jupiter were composed entirely of solid materials sufficiently strong to resist all crushing strains to which they may be exposed, and that its form, in the absence of all disturbing forces, were an exact sphere. Such a body, turning on an axis perpendicular to the plane of its orbit and in

* Communicated by the Author.

a time different from that of its revolution, would have its equatorial gravity subject to a variation of about $1\frac{2}{3}$ per cent.; and the central pressure along the plane of the equator would undergo a periodical change of about 3000 pounds to the square inch. Now in consequence of the compressibility which belongs to every kind of solid matter, the satellite would be continually changed to an ellipsoid, the longest diameter always forming the same angle with the direction of the primary. If its component parts had a modulus of elasticity as great as that of iron, a difference of one-fifth of a mile may be expected between the major and mean axis; but were the mass of a more yielding character, or were it covered with fluid, its ever-changing form would deviate more considerably from a true sphere.

The effect which the attraction of the primary would exert on the rotation of an ellipsoidal satellite, the major axis of which had a constant inclination to the radius vector of its orbit, may be found by a method similar to that pursued for determining theoretically the amount of the precession of the equinoxes. Let A, B, and C be the major, mean, and minor semiaxes of the ellipsoid, the last being perpendicular to the plane of the orbit, and the first forming the angle ψ with the direction of the primary. Supposing the satellite homogeneous, the change in the velocity of rotation at the extremity of the major axis in a unit of time will be expressed by

$$\frac{3M}{2D^3}(A-B)\sin 2\psi, \quad . \quad . \quad . \quad . \quad . \quad (1)$$

D being the distance of the primary, and M the measure of its attractive power. According to the theory of central forces, $\frac{M}{D^3}=\frac{4\pi^2}{T^2}$, π being put for 3·1416, and T for the time of revolution; the expression for the change in the equatorial movement thus becomes

$$\frac{6\pi^2(A-B)}{T^2}\sin 2\psi. \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Now, for a synchronism of the orbital and diurnal motions, the equator must have a velocity equal to $\frac{2\pi A}{T}$; and dividing this by the last expression, there results

$$T'=\frac{TA}{3\pi(A-B)\sin 2\psi}, \quad . \quad . \quad . \quad . \quad (3)$$

T' denoting the time in which a satellite, having no primitive rotation, would acquire one sufficiently rapid for keeping the

same point of its surface in perpetual conjunction with the primary.

In the case of a solid satellite composed of imperfectly elastic materials, it is necessary to take into consideration the slight change of density attending the constant alteration of form. Had this been done, the expressions (1) and (2) would be reduced to four-fifths of their value; while instead of formula (3) we should find

$$T' = \frac{5TA}{12\pi(A-B)\sin 2\psi}. \quad . \quad . \quad . \quad . \quad (4)$$

If the angle ψ were equal to 90 degrees, no change would be indicated in the rotation; but the angle could not have this magnitude except in the case of a solid satellite all parts of which were perfectly elastic, or in the case of one, consisting wholly or partially of fluid, which performed its tidal oscillations without friction.

By another investigation, which brevity compels me to omit, I have arrived at the same results in regard to the secular changes which the rotation of a secondary body must experience until it keeps pace with the orbital revolution. It will also readily appear that the ultimate effect of these changes is not affected by the inclination of the equator of the satellite to the plane of its orbit. But it will be necessary to show that the inclination is doomed to undergo a slow permanent diminution when the synchronism of the rotation and revolution is once established. For this purpose we may proceed in a manner similar to that employed in investigating the mutation of the earth's axis. Let I be the inclination of the equator of the satellite to the plane of its orbit, which for simplicity may be regarded as circular, and let L be the longitude of the satellite reckoned from the point of their intersection. Regarding the body as an ellipsoid, the tendency of the disturbing force to move the axis towards the plane of the orbit will be

$$\frac{3M}{2D^3}(A-C)\sin^2 I \sin 2L. \quad . \quad . \quad . \quad . \quad (5)$$

If δI denote the change of inclination from this cause, then

$$\frac{d^2\delta I}{dt^2} = \frac{3M}{2D^3}\left(\frac{A-C}{A}\right)\sin^2 I \sin^2 L. \quad . \quad (6)$$

On substituting nt for L, and regarding A and C as constant, the integration will give only periodical quantities; so that no permanent change would be indicated if the form were absolutely immutable. But supposing A − C to vary, either from the presence of large collections of fluid on the surface of the satellite, or from the necessary elasticity of its solid matter, the quantity

$\frac{A-C}{A}$ in the last equation must receive an increment, the principal term of which will be $C\cos(2L-2w)$, or

$$C(\cos 2L\cos 2w+\sin 2L\sin 2w), \quad . \quad . \quad . \quad (7)$$

in which w represents the increase of longitude during the interval between the times of high tides at any locality, and of the maximum intensity of tidal force. Denoting by N and N′ the sine and cosine of w, which is constant, formula (6) becomes

$$\frac{d^2\delta I}{dt^2}=\frac{3M}{D^3}\left(\frac{A-C}{A}\sin^2 I\sin 2L+\frac{CN'}{2}\sin^2 I\sin 4L\right.$$
$$\left.-\frac{CN\sin^2 I\cos 4L}{2}+\frac{C\sin^2 IN}{2}\right). \quad . \quad . \quad . \quad . \quad . \quad (8)$$

If this equation be integrated, all the resulting terms of the second member will be periodical except the last, which will express the slow permanent diminution of I; but the term will disappear when w is exactly 90 degrees, as it should be if the oscillations on which the change of form depended were effected without any loss of force.

Although the influence of distant bodies in changing the plane of the orbit may prevent I from sinking to zero, yet we must recognize the tendency to the peculiar arrangement which reduces to the lowest scale the dynamic effects of the disturbing force on their surfaces of secondary planets. But though their times of rotation and the position of their axes may be adjusted for attaining this object, the eccentricity of the orbit would bring tidal action into existence; and any commotions which this might occasion in their seas must be attended with secular changes in the size and form of their orbits. This will appear evident when we consider that these tides could not reach their higheat level on the parts of the satellite in conjunction with the primary, until some time after the disturbing force which produced them attained its greatest intensity; and the subordinate world would thus present a greater deviation from a true sphere, in passing from the lower to the higher apsis, than in returning to the former point. It would accordingly feel the restraint of the centripetal force more intensely when retiring from the primary than when approaching him; and its motion would be retarded during the former period to an extent slightly greater than that to which it is accelerated during the latter. We may therefore reasonably expect a secular alteration in its mean motion and the size of its orbit; but it may be advisable to show by analytical investigations, that such changes take place on a scale corresponding to the waste of tidal power.

Although this may be done without any hypothesis in regard

to form and density, yet we may more easily arrive at definite results by taking, as the most appropriate type of the figure of these bodies, the ellipsoid which a homogeneous fluid satellite must assume when its motions are adapted for keeping the same point of its surface always directed to the centre of the primary. Let A, B, and C represent the semiaxes; P, Q, and R the attractions at their extremities in the absence of all disturbing influences; and put $\frac{A^2-C^2}{A^2}=\epsilon^2$ and $\frac{B^2-C^2}{B^2}=\epsilon_1^2$. By a course of investigation similar to that which I adopted for finding the attraction of a prolate spheroid in the Philosophical Magazine (vol. xx. p. 414) the following result may be obtained:—

$$P=\frac{2gk^2C^2}{A}\iint\frac{\cos^2\phi\sin\phi d\phi d\theta}{1-\epsilon^2\cos^2\phi-\epsilon_1^2\sin^2\phi\cos^2\theta}, \quad . \quad (9)$$

in which g denotes the attractive force at the distance k of a small portion of the body, ϕ the angle formed with the axis A by any of the elementary pyramids extending from its extremity to the surface of the ellipsoid, and θ the angle which the projection of these pyramids on the plane of B and C forms with B. A double integration by series, rejecting the fourth and higher powers of ϵ and ϵ_1, gives

$$P=\frac{4\pi gk^2C^2}{3A}\left(1+\frac{3\epsilon^2}{5}+\frac{\epsilon_1^2}{5}\right). \quad . \quad . \quad . \quad (10)$$

In like manner, by a slight modification of the process employed in the same article (page 415) for finding the attraction at the extremity of the minor axis of the prolate spheroid, we may obtain

$$Q=\frac{4\pi gk^2C^2}{3B}\left(1+\frac{\epsilon^2}{5}+\frac{3\epsilon_1^2}{5}\right), \quad . \quad . \quad . \quad (11)$$

$$R=\frac{4\pi gk^2C}{3}\left(1+\frac{\epsilon^2}{5}+\frac{\epsilon_1}{5}\right). \quad . \quad . \quad . \quad (12)$$

Let P′, Q′, and R′ represent the actual intensity of gravity at the extremity of each axis, taking into consideration the effects of centrifugal force and the disturbance of the primary, to which the axis A is always directed, while C is perpendicular to the plane of the orbit; for this condition is necessary for the equilibrium, as I have shown in the Philosophical Magazine for April 1861. Then

$$P'=\frac{4\pi gk^2C^2}{3A}\left(1+\frac{3\epsilon^2}{5}+\frac{\epsilon_1^2}{5}\right)-\frac{4\pi gk^2nh^3A}{D^3}, \quad . \quad (13)$$

$$Q'=\frac{4\pi gk^2C^2}{3^2B}\left(1+\frac{\epsilon^2}{5}+\frac{3\epsilon_1^2}{5}\right), \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

$$R'=\frac{4\pi gk^2C}{3}\left(1+\frac{\epsilon^2}{5}+\frac{\epsilon_1^2}{5}\right)+\frac{4\pi gk^2nh^3C}{3D^3}, \quad . \quad . \quad (15)$$

in which h denotes the radius of the primary supposed to be a sphere, D its distance, and n its density divided by that of the satellite. In the article just referred to, it has been shown that P′, Q′, and R′ must be reciprocally proportional to A, B, and C; and accordingly by equalling the values of AP′, BQ′, and CR′ as deduced from the last equation, we obtain

$$\epsilon^2 = \frac{10nh^3}{D^3}, \quad \epsilon_l^2 = \frac{5nh^3}{D^3}; \quad . \quad . \quad . \quad . \quad (16)$$

whence

$$\epsilon_l^2 = \frac{\epsilon^2}{4} \text{ and } \frac{A-C}{A} = 4\left(\frac{B-C}{B}\right). \quad . \quad . \quad (17)$$

If the satellite were not homogeneous, the ratio between the greatest and least ellipticities would vary between 4 and 6, the latter number expressing the ratio in the case in which the central matter alone is supposed to be endued with attractive power.

The extent to which the form of the satellite affects the intensity of the force which binds it to the primary, supposing this body to be a sphere, may be readily found by means of Ivory's theorem; and the application will be facilitated in the present case, in which the external point ranges with the axis A. The effect of the attractive force in moving the primary will be

$$\frac{4\pi k^2 g \mathrm{ABC}}{3D^2}\left(1 + \frac{3}{5}\frac{A^2\epsilon^2}{D^2} - \frac{3}{10}\frac{A^2\epsilon_l^2}{D^2}\right). \quad . \quad . \quad (18)$$

But the same amount of matter in a spherical form would attract the central orb with a force expressed by $\frac{4\pi k^2 g \mathrm{ABC}}{3D^2}$; so that the excess of attractive power due to the ellipticity is

$$\frac{4\pi k^2 g \mathrm{ABC}}{3D^4}\left(\frac{3}{5}A^2\epsilon^2 - \frac{3}{10}A^2\epsilon_l^2\right). \quad . \quad . \quad (19)$$

Calling this F, and putting m for $\frac{4\pi k^2 g \mathrm{ABC}}{3}$, and $\frac{\epsilon^2}{4}$ for ϵ_l^2, in accordance with formula (17),

$$\mathrm{F} = \frac{21}{40}\frac{mA^2\epsilon^2}{D^4}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (20)$$

To show the effects of the change of form in consequence of the eccentricity of the orbit, which is to be regarded as deviating little from a circle, we must take the variation of the last formula. Then

$$\delta\mathrm{F} = \frac{21}{20}\frac{mA\epsilon}{D^4}\left(A\delta\epsilon + \epsilon\delta A - \frac{2A\epsilon\delta D}{D}\right). \quad . \quad . \quad (21)$$

But the volume of the ellipsoid is equal to $A^3\left(\frac{1-\epsilon^2}{\sqrt{1-\epsilon_l^2}}\right)$ or

$A^3\left(1-\frac{7}{8}\epsilon^2\right)$ nearly, from which we obtain

$$A\delta\epsilon=\frac{12\delta A}{7\epsilon} \text{ nearly;}$$

and formula (19) becomes

$$\delta F=\frac{9mA\delta A}{5D^4}+\frac{21}{20}\frac{mA\epsilon^2\delta A}{D^4}-\frac{21}{10}\frac{mA^2\epsilon^2\delta D}{D^5}. \quad (22)$$

δA is the change of level at the extremities of the major axis arising from the variation of the primary disturbance; and regarding these tides as conforming to dynamic principles, their maximum range must be proportional to the force producing them, multiplied by the square of its time of operation. As the force in these cases varies inversely as the fourth power of the distance, while the square of the time, according to Kepler's third law, is directly proportional to the cube of the same quantity, the maximum value of δA may be represented by $\frac{Se}{D}$. Now, if W be the angle which the satellite describes during the time the tidal force requires to produce its full effects, v being the true anomaly reckoned from the higher apsis, and e the relative eccentricity of the orbit, then

$$\delta A=-\frac{Se}{D}\cos(v-W). \quad (23)$$

Substituting this value for δA, and for δD its approximate value $D_1 e\cos v$, formula (22) becomes

$$\delta F=-\frac{9mASe\cos v-W}{5D^5}-\frac{21}{10}\frac{mA^2\epsilon^2eD_1\cos v}{D^5}, \quad (24)$$

the middle term of the second member being rejected as inconsiderable, and D_1 denoting the mean distance.

Formula (18) expresses the attractive force of the satellite on the primary supposed to be a sphere; F is the extent to which this force is augmented by the ellipticity of the satellite, and δF is the periodical change in the value of F in consequence of tidal fluctuations. The second term of the value of δF in equation (24) would be the same if the body were entirely solid; and accordingly it could not be expected to lead to non-periodical alterations in the orbit, but it has been retained to show that analysis leads to the same conclusion. Now to express the effect of these forces on the orbit which the satellite describes around the centre of the primary, the values of F and δF must be multiplied by $\frac{M+m}{m}$, M being the measure of the attractive energy

of the central sphere. The disturbing force on the orbit thus becomes

$$\frac{M's}{r^4} - \frac{Mse\cos(v-W)}{r^5} - \frac{Ms''e\cos v}{r^5}, \quad . \quad . \quad (25)$$

M′ being put for (M+m), r for D, s for $\frac{21A^2\epsilon^2}{40}$, s' for $\frac{9AS}{5}$, and s'' for $\frac{21A^2\epsilon^2D_1}{10}$. Accordingly, in the problem of the two bodies, the differential equation $\frac{d^2r}{dt^2}=\frac{a^2}{r^3}-\frac{M'}{r^2}$ becomes in the present case

$$\frac{d^2r}{dt^2}=\frac{a^2}{r^3}-\frac{M'}{r^2}-\frac{M's}{r^4}+\frac{M's'e}{r^5}\cos(v-w)+\frac{M's''e}{r^5}\cos v. \quad . \quad (26)$$

Multiplying by dr and integrating,

$$\frac{dr^2}{dt^2}=-b-\frac{a^2}{r^2}+\frac{2M}{r}+\frac{2}{3}\frac{M's}{r^3}+2M's'e\int\frac{\cos(v-w)dr}{r^5}$$

$$+2M's''e\int\cos v\,dr. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (27)$$

In order to effect the integration of the last terms, which are extremely minute, we may substitute the elliptical values of r and dr in them. The last term integrated in this way gives only periodical quantities. But

$$\int\frac{\cos v-w}{r^5}dr=-\int\frac{e\sin v(1-e\cos v)^3}{p^4}(\cos v\cos W+\sin v\sin W)dv, \quad (28)$$

p being the parameter of the orbit. Now the angle W being invariable, its sine and cosine may be expressed by the constant quantities H and H′; and the term becomes

$$-e\mathrm{H}'\int\frac{\sin v\cos v(1-e\cos v)^3dv}{p^4}-e\mathrm{H}\int\frac{\sin^2v(1-e\cos v)^3}{p^4}dv. \quad (29)$$

It may be readily found that the first integral consists wholly of quantities multiplied by cosines of v and its multiples, and therefore periodical, while the second is equivalent to

$$-\frac{e\mathrm{H}}{2p^4}\int(1-\cos 2v)(1-e\cos v)^3dv, \quad . \quad . \quad . \quad . \quad (30)$$

the secular part of which is found to be

$$-\frac{e\mathrm{H}v}{2p^4}\left(1+\frac{e^2}{2}\right) \text{ or } -\frac{e\mathrm{H}v}{2p^4} \text{ nearly.} \quad . \quad . \quad . \quad (31)$$

Equation (27) thus becomes

$$\frac{dr^2}{dt^2}=\frac{2M}{r}-\frac{a^2}{r^2}-b+\frac{2}{3}\frac{M's}{r^3}-\frac{M's'e^2\mathrm{H}v}{p^4}+\beta, \quad . \quad (32)$$

β denoting the periodical quantities arising from the disturbance. Since $\frac{2M's}{3r^3}$ is extremely minute, it may be replaced by $\frac{2M's}{3D_1^3}$, D_1 being the mean distance. The maximum and minimum values of r may then be found by a quadratic on making $\frac{dr^2}{dt^2}=0$, and omitting the periodical terms denoted by β. From the coefficient of r in the resulting equation, it appears that

$$D_1=\frac{M'}{b}-\frac{M^2}{b^2}\left(\frac{2D}{3D_1^3}+\frac{s'eHv}{p^4}\right). \quad . \quad . \quad . \quad (33)$$

$\frac{M'}{b}$ being the value of the mean distance in the absence of the disturbance, and s' being equal to $\frac{9AS}{5}$, the last expression becomes

$$D_1=\text{Constant}-\frac{9AD^2Sev}{5p^4}\sin W; \quad . \quad . \quad . \quad (34)$$

and the secular diminution of D_1 during each revolution will be

$$\frac{5{\cdot}6548Az\sin W}{D_1}\text{ nearly}, \quad . \quad . \quad . \quad . \quad (35)$$

in which z denotes the highest swell of the tides at the points of the satellite in conjunction and in opposition with the primary. The diminution which the disturbance occasions during the same period in the relative eccentricity or the eccentricity divided by D_1 will be

$$\frac{2{\cdot}8274Az\sin W}{D_1^2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (36)$$

These results may also be obtained by investigating the variation of the elements of the orbit according to the method of Lagrange. If the tides could rise and fall on a satellite without any impediments from friction, W would become equal to 180 degrees, and there could be no permanent change in the ellipse which the body describes. It thus appears that the duration of the secondary planets is much dependent on the absence of tides from their surfaces; and perhaps the vast number of these attendants belonging to the remote planets may be indebted for their present existence to the intense cold, which keeps their oceans in a perpetually frozen condition.

Cincinnati, November 8, 1861.

LXIV. *Chemical Analysis by Spectrum-observations.*

By G. Kirchhoff *and* R. Bunsen.

[With a Chromolithograph Plate.]

[Continued from p. 349.]

IV. *On Metallic Cæsium and some of its Compounds.*

a. *Metallic Cæsium.*

If fused chloride of cæsium be placed in the circuit of a powerful zinc-carbon battery, exactly the same phenomena are noticed as when the chlorides of potassium or rubidium are thus treated.

The amalgam of cæsium is, however, not so easily formed from an aqueous solution of the chloride as is the rubidium-amalgam under similar circumstances. It can be obtained in a solid crystalline form only by the aid of a very powerful current. When thus prepared it is of a silver-white colour, exhibiting a granular structure. It undergoes oxidation on exposure to air much more rapidly than rubidium-amalgam, and quickly decomposes water. With a solution of chloride of potassium, it is found to be positively electric when compared with the amalgams of sodium, potassium, and rubidium; so that cæsium must be considered as the most electro-positive of all the known elementary bodies.

b. *Hydrated Oxide of Cæsium.*

The properties of fused chloride of cæsium, when acting as an electrolyte, show plainly that, like potassium, this metal forms a suboxide. We have not yet examined the compounds formed by cæsium with more than one atom of oxygen; the analogy of the metal with potassium would, however, render the existence of such compounds probable. The hydrated oxide, which is prepared in a similar manner to the corresponding rubidium compound, resembles the latter in all its properties. It contains one atom of water, which cannot be expelled by heat; it is in a high degree deliquescent, becomes strongly heated in contact with water, and is at least as powerful a caustic as potash or hydrated oxide of rubidium. It dissolves easily in alcohol, forming a syrupy liquid.

c. *Monocarbonate of Cæsium.*

Like the corresponding rubidium compound, this salt is most easily obtained by decomposing the boiling solution of the sulphate of cæsium with baryta-water, evaporating the caustic liquor to dryness with carbonate of ammonium, and separating any insoluble carbonate of barium by filtration. From the syrupy solution of the carbonate, the hydrated salt crystallizes in irregular masses, which soon deliquesce on exposure. The crystals, on heating, fuse in their water of crystallization, leaving a residue

of the anhydrous salt in the form of a sandy friable white mass, which rapidly absorbs moisture from the air. At a red heat the anhydrous salt melts; and it may be heated to whiteness, at which temperature it begins to volatilize, without losing carbonic acid. Placed on a platinum wire in the flame, it soon volatilizes completely. The aqueous solution of the salt possesses a strong alkaline reaction and taste; when rubbed between the fingers, it produces the peculiar soapy feeling characteristic of the alkalies, and it acts as a cautery when it is allowed to remain for some time in contact with the skin. Water containing $\frac{1}{10000}$th part of the salt turns red litmus-paper distinctly blue.

Monocarbonate of cæsium possesses a property which is remarkable in the alkaline carbonates, that, namely, of solubility in absolute alcohol. 100 parts of alcohol dissolve, at 19° C., 11·1, and at the boiling-point of the alcohol 20·1 parts of this salt. The carbonate can be obtained in the form of small irregular crystals by quickly cooling the alcoholic solution. If the cooling be carried on slowly to temperatures below 0° C., the salt sometimes separates out in tabular crystals often 1 inch in length, especially if some quantity of caustic oxide of cæsium be present. 0·7921 grm. of the fused salt lost, on treatment with dilute sulphuric acid, 0·1120 grm. carbonic acid. Hence the salt contains—

		Calculated.	Found.
CsO . . .	131·35	85·65	85·86
CO^2 . . .	22·00	14·35	14·14
	153·35	100·00	100·00

d. *Bicarbonate of Cæsium.*

A solution of monocarbonate of cæsium, exposed in an atmosphere of carbonic acid, passes into this salt in the course of a few days. The solution, on standing in the air at the ordinary temperature over sulphuric acid, deposits large but indistinctly formed striated crystals, which are unalterable in the air, and assume a prismatic form; they possess a feeble alkaline reaction; their aqueous solution gives off carbonic acid on boiling, and in outward properties they cannot be distinguished from the crystals of the corresponding rubidium salt. 0·8155 grm. of fused monocarbonate of cæsium yielded 0·9761 grm. of bicarbonate when exposed for some days in an atmosphere of carbonic acid, and afterwards dried over sulphuric acid. Hence the composition of the salt is—

		Calculated.	Found.
CsO . . .	131·35	71·25	71·56
$2CO^2$ . . .	44·00	23·87	28·44
HO . . .	9·00	4·88	
	184·35	100·00	100·00

e. *Nitrate of Cæsium.*

This salt contains no water of crystallization, it does not undergo alteration in the air, and may be obtained from its aqueous solution in the form of small shining crystals of a prismatic form, in which the faces of the prism are generally better defined than those at the summits. The crystals obtained by slow evaporation at 14° C. belong to the hexagonal system, and are isomorphous with nitrate of rubidium. The primary form is an obtuse hexagonal dodecahedron, with polar angles of 142° 56′, and basal angles of 78° 58′, corresponding to the following relation of the axes

$$1 : a = 1 : 0{\cdot}71348.$$

The faces which could be observed (see Plate V. fig. 1) are as follows:—

P . ∞P . P2 . ∞P2 . 0P . ¾P.
r *p* r_1 p_1 *o* *q*.

	Calculated.	Found.
$p-p_1$ . . .	150° 0′	149° 59′
p_1-p . . .	150 0	149 58
$r-p_1$* . .		129 29
r_1-p_1 . .	125 30	125 28
$r-r_1$ . .	161 28	161 41
$r-q$ . .	172 14	172 0
r_1-o . .	144 30	144 39

If the primary form be taken to be a hexagonal dodecahedron of the second order, the corresponding hexagonal dodecahedron of the first order yields as a hemihedral form a rhombohedron having polar angles of 106° 40′. Through this form, therefore, the isomorphism of the nitrates of cæsium and rubidium, and the potash and soda nitre, becomes apparent. We have—

Nitrate of cæsium	106° 40′
Nitrate of potassium . . .	106 30
Nitrate of sodium	106 36

When crystallized quickly, the salt separates out in long needle-shaped prisms, longitudinally striated. It has the same saline bitter cooling taste as saltpetre—so much so that these salts cannot thus be distinguished from each other. On heating, the salt melts to a thin liquid at temperatures almost below the red heat; and when more strongly heated it evolves oxygen, and is converted first into nitrite, and afterwards, by absorption of moisture from the air, into caustic hydrate of cæsium, which

* This angle served as basis of calculation for the primary form.

attacks glass and platinum. In absolute alcohol the salt is very slightly soluble*.

Nitrate of cæsium is somewhat more difficultly soluble in water than the corresponding potassium compound; for whilst 100 parts of water at $+3°{\cdot}2$ C. dissolve 16·1 parts of the latter, 10·58 parts of nitrate of cæsium are dissolved under similar circumstances.

3·0567 grms. of pure nitrate of cæsium gave 2·8233 grms. of the sulphate on decomposition and ignition with sulphuric acid. Hence the composition of the salt is—

		Calculated.	Found.
CsO . . .	131·35	70·87	70·80
NO^5 . . .	54·00	29·13	29·20
	185·35	100·00	100·00

f. *Bisulphate of Cæsium.*

Carbonate of cæsium is gradually heated with an excess of sulphuric acid until the temperature rises nearly to redness. The salt then consists of a transparent colourless liquid, which, on cooling, solidifies to a crystalline mass. Dissolved in water, the acid salt thus obtained crystallizes upon slow evaporation in the form of small short rhombic prisms, having rectangular terminations, and having the acute longitudinal edges equally bevelled. The crystals belong to the rhombic system. The relation of the horizontal axes is nearly

$$a : b = 1 : 1{\cdot}38.$$

The crystals obtained were badly formed, and their surfaces were not polished enough to enable us to make any accurate measurements with the reflecting goniometer. The relation of the principal axis to the horizontal axes could also not be obtained, as no faces were visible on the terminal edges of the prism. The crystals are represented by fig. 3, Plate V.

	Found.	Calculated.
$p-p$ on a . . .	$107° 37'$	$108°$
$p-b$	126	

The salt has a strongly acid reaction and taste; it is, however, unalterable in the air. Heated gently, it melts quietly under a red heat; and when more strongly ignited, sulphuric anhydride escapes with effervescence, leaving a solid mass of neutral sulphate of cæsium, which melts at a temperature approaching a yellow heat.

* Saltpetre is by no means insoluble in alcohol, as Berzelius affirms. The slight solubility of the cæsium nitrate in alcohol cannot, therefore, be used, as one of us formerly proposed, as a distinctive reaction of these two salts.

g. *Neutral Sulphate of Cæsium.*

The aqueous solution of this salt possesses an insipid taste, but a bitter after-taste. It is far more soluble in water than the corresponding potassium-salt. 100 parts of water take up at $-2°$ C. not less than 158·7 parts of sulphate of cæsium, whereas only 8·0 parts of sulphate of potassium are dissolved under similar circumstances. When the aqueous solution is allowed to evaporate slowly over sulphuric acid, small, irregularly formed, hard crystals are deposited, which generally are found to have the form of short flattened prisms, and often occur grouped together in irregular masses. The crystals are anhydrous, quite unaffected by exposure to air, and insoluble in alcohol. We have not succeeded in obtaining any individual crystals suitable for measurement.

The analysis of the salt was made by converting the carbonate into the sulphate. For this purpose 0·7921 grm. of fused carbonate of cæsium was treated with sulphuric acid, and yielded 0·8828 grm. of fused sulphate. Hence the composition of the salt is—

		Calculated.	Found.
CsO . . .	131·35	76·66	76·85
SO^3 . . .	40·00	23·34	23·15
	171·35	100·00	100·00

With the sulphates of cobalt, nickel, magnesium, &c., sulphate of cæsium produces a series of beautifully crystallizing double salts, containing 6 atoms of water of crystallization, and isomorphous with the corresponding salts of potassium and rubidium. The following faces were observed in these crystals:—

$$0P\,.\,\infty P\,.\,+P\,.\,[P\,\infty]\,.\,+2P\infty\,.\,\infty P2.$$

The sulphate of cæsium also forms with sulphate of aluminium a double salt containing 24 atoms of water, and crystallizing in the regular system, corresponding exactly to potassium and rubidium alums.

h. *Chloride of Cæsium.*

By neutralizing the carbonate with hydrochloric acid and evaporating the solution, chloride of cæsium is obtained in the form of small anhydrous indistinct cubes. When quickly crystallized, the salt appears, like sal-ammoniac and chloride of potassium, as a mass of feathery crystals. Chloride of cæsium fuses at a low red heat, and volatilizes at a higher temperature much more easily than chloride of potassium, in the form of white vapours. The fused salt, on cooling, assumes the form of a white opake mass, which rapidly absorbs moisture from the air

and deliquesces. When ignited for a long time in contact with air it becomes slightly basic.

According to the atomic-weight determinations, already described, 1·0124 grm. of chloride of cæsium, the solution of which was perfectly neutral, yielded 0·9133 grm. chloride of silver, and 0·0009 grm. of metallic silver from the filter-ash. This corresponds to the following numbers:—

		Calculated.	Found.
Cs	123·35	77·67	77·67
Cl	35·46	22·33	22·33
	158·81	100·00	100·00

i. *Double Chloride of Platinum and Cæsium.*

If, to an aqueous solution of chloride of cæsium, bichloride of platinum be added, a yellow precipitate is formed. The colour of this is somewhat lighter than that of the corresponding potassium-salt, because it is less soluble than the latter, and therefore is deposited in a finer state of division. The precipitate is anhydrous, and is composed of microscopic, honey-yellow, transparent regular octahedrons. 100 parts of water dissolve of this compound—

at 0° C.	0·021 part.	at 68° C.	0·234 part.
11	0·072 „	100	0·382 „
40	0·118 „		

These numbers are taken from the mean of a large number of careful determinations agreeing well amongst themselves.

As almost all the platinum which is found in commerce is very impure, and often possesses an atomic weight from 6 to 8 per cent. below the true value, we have previously purified the platinum which we used for the preparation of these as well as of the rubidium salts. This purification was effected by fusing the chloride of platinum and potassium in a platinum dish with a mixture of the carbonates of potassium and sodium, washing out the mass with water, and dissolving the residue in dilute aqua regia. When this operation had been repeated five times, it was found that the platinum attained an atomic weight varying but very slightly from 99·1.

The analysis of the double chloride was carried out as follows:—The salt was weighed out in a U-shaped tube of hard glass, after having been dried in a bath of fused chloride of zinc at a temperature of 160° to 170° C., and the tube, with the substance, bedded in magnesia and heated to dull redness, whilst a current of dry hydrogen was passed over the salt. The loss of weight thus obtained was determined, the chloride of cæsium

separated by boiling with water from the insoluble platinum, both substances weighed, and the chlorine in the chloride of cæsium estimated with silver.

Experiment gave—

Chloride of platinum and cæsium employed.	8·6142 grms.
Loss on reduction with hydrogen	1·8725 „
Platinum separated	2·6138 „
Chloride of cæsium obtained	4·1544 „
Chloride of silver obtained	3·7506 „

Hence we obtain the following composition:—

			Calculated.	Found.
Bichloride of platinum	Pt.	99·10	30·14	30·25
	Cl^2	70·92	21·57	21·67
Chloride of cæsium	Cs	123·35	37·51	37·35
	Cl.	35·46	10·78	10·53
		328·83	100·00	99·80

It is interesting to compare the solubility of the double chlorides of platinum, rubidium, and cæsium with the potassium-platinum double salt. The solubility of the latter is seen from the following experiments, which were conducted with special care, the numbers being the mean of several well-agreeing determinations.

100 parts of water dissolve—

at 0° C.	0·724	chloride of platinum and potassium.
6·8	0·873	„ „
13·8	0·927	„ „
46·5	1·776	„ „
71·0	3·018	„ „
100·0	5·199	„ „

By interpolation, the solubility of the cæsium, rubidium, and potassium-platinum chlorides is obtained for intervals of 10° C., and is found to be as follows:—

	Potassium-salt.	Rubidium-salt.	Cæsium-salt.
0° C.	0·74	0·184	0·024
10	0·90	0·154	0·050
20	1·12	0·141	0·079
30	1·41	0·145	0·110
40	1·76	0·166	0·142
50	2·17	0·203	0·177
60	2·64	0·258	0·213
70	3·19	0·329	0·251
80	3·79	0·417	0·291
90	4·45	0·521	0·332
100	5·18	0·634	0·377

V. *Reactions of the Rubidium and Cæsium Compounds.*

Cæsium and rubidium are not precipitated either by sulphuretted hydrogen or by carbonate of ammonium. Hence both metals must be placed in the group containing magnesium, lithium, potassium, and sodium. They are distinguished from magnesium, lithium, and sodium by their reaction with bichloride of platinum, which precipitates them like potassium. Neither rubidium nor cæsium can be distinguished from potassium by any of the usual reagents. All three substances are precipitated by tartaric acid as white crystalline powders; by hydrofluosilicic acid as transparent opalescent jellies, and by perchloric acid as granular crystals; all three, when not combined with a fixed acid, are easily volatilized on the platinum wire, and they all three tinge the flame violet. The violet colour appears indeed of a bluer tint in the case of potassium, whilst the flame of rubidium is of a redder shade, and that of cæsium still more red. These slight differences can, however, only be perceived when the three flames are observed side by side, and when the salts undergoing volatilization are perfectly pure. In their reactions, then, with the common chemical tests, these new elements cannot be distinguished from potassium. The only method by means of which they can be recognized when they occur together is that of spectrum-analysis.

The spectra of rubidium and cæsium are highly characteristic, and are remarkable for their great beauty. In examining and measuring these spectra we have employed an improved form of apparatus, which in every respect is much to be preferred to that described in our first memoir. In addition to the advantages of being more manageable and producing more distinct and clearer images, it is so arranged that the spectra of two sources of light can be examined at the same time, and thus, with the greatest degree of precision, compared, both with one another and with the numbers on a divided scale.

The apparatus is represented by fig. 12, Plate VI. On the upper end of the cast-iron foot F a brass plate is screwed, carrying the flint-glass prism P, having a refracting angle of 60°. The tube A is also fastened to the brass plate; in the end of this tube, nearest the prism, is placed a lens, whilst the other end is closed by a plate in which a vertical slit has been made. Two arms are also fitted on to the cast-iron foot, so that they are moveable in a horizontal plane about the axis of the foot. One of these arms carries the telescope B, having a magnifying power of 8, whilst the other carries the tube C; a lens is placed in this tube at the end nearest to the prism, and at the other end is a scale which can be seen through the telescope by

reflexion from the front surface of the prism. This scale is a photographic copy of a millimetre-scale, which has been produced in the camera, of about $\frac{1}{13}$ the original dimensions*. The scale is covered with tinfoil so that only the narrow strip upon which the divisions and the numbers are engraved can be seen.

The upper half only of the slit is left free, as is seen by reference to fig. 11, Plate VI.; the lower half is covered by a small equilateral glass prism, which sends by total reflexion the light of the lamp D, fig. 12, through the slit, whilst the rays from the lamp E pass freely through the upper and uncovered half. A small screen placed above the prism, prevents any of the light from D passing through the upper portion of the slit. By help of this arrangement the observer sees the spectra of the two sources of light immediately one under the other, and can easily determine whether the lines are coincident or not†.

We now proceed to describe the arrangement and mode of using the instrument.

The telescope B is first drawn out so far that a distant object is plainly seen, and screwed into the ring, in which it is held, care being taken to loosen the screws α and β beforehand. The tube A is then brought into its place, and the axis of B brought into one straight line with that of A. The slit is then drawn out until it is distinctly seen on looking through the telescope, and this latter is then fixed by moving the screws α and β, so that the middle of the slit is seen in about the middle of the field of view. After removing the small spring γ, the prism is next placed on the brass plate, and fastened in the position which is marked for it, and secured by screwing down the spring γ. If the axis of the tube A be now directed towards a bright surface, such as the flame of a candle, the spectrum of the flame is seen in the lower half of the field of the telescope on moving the latter through a certain angle round the axis of the foot F. When the telescope has been placed in position, the tube C is fastened on to the arm belonging to it, and this is turned through an angle round the axis of the foot such that, when a light is allowed to fall on the divided scale, the image of the scale is seen through the telescope B, reflected from the nearer face of the prism. This image is brought

* This millimetre-scale was drawn on a strip of glass covered with a thin coating of lampblack and wax dissolved in glycerine. The divisions and the numbers, which by transmitted light showed bright on a dark ground, were represented in the photograph dark on a light ground. It would be still better to employ, for the spectrum-apparatus, a scale in which the marks were light on a dark ground. Such scales are beautifully made by Salleron and Ferrier of Paris.

† This apparatus was made in the celebrated optical and astronomical atelier of C. A. Steinheil in Munich.

exactly into focus by altering the position of the scale in the tube C; and by turning this tube on its axis, it is easy to make the line in which one side of the divisions on the scale lie, parallel with the line dividing the two spectra, and by means of the screw δ to bring these two lines to coincide.

In order to bring the two sources of light, D and E, into position, two methods may be employed. One of these depends upon the existence of bright lines in the inner cone of the colourless gas-flame, which have been so carefully examined by Swan. If the lamp E be pushed past the slit, a point is easily found at which these lines become visible; the lamp must then be pushed still further to the left, until these lines nearly or entirely disappear; the right mantle of the flame is now before the slit, and into this the bead of substance under examination must be brought. In the same way the position of the source of light D may be ascertained.

The second method is as follows:—The telescope B is so placed that the brightest portion of the spectrum of the flame of a candle is seen in about the middle of the field of view; the flame is then placed before the ocular in the direction of the axis of the telescope, and the position before the slit determined in which the upper half of the slit appears to be the brightest; the lamp E is then placed so that the slit appears behind that portion of the flame from which the most light is given off after the introduction of the bead. In a similar way the position of the lamp D is determined by looking through the small prism and the lower half of the slit.

By means of the screw ϵ, the breadth of the slit can be regulated in accordance with the intensity of the light, and the degree of purity of spectrum which is required. To cut off foreign light, a black cloth, having a circular opening to admit the tube C, is thrown over the prism P and the tubes A and B. The illumination of the scale is best effected by means of a luminous gas-flame placed before it; the light can, if necessary, be lessened by placing a silver-paper screen close before the scale. The degree of illumination suited to the spectrum under examination can then be easily found by placing this flame at different distances.

In order to obtain representations of the spectra of cæsium and rubidium corresponding to those of the other metals which we have given in our former paper, we have adopted the following course:—

We placed the tube C in such a position that a certain division of the scale, viz. No. 100, coincided with Fraunhofer's line "D" in the solar spectrum, and then observed the position of the dark solar lines A, B, C, D, E, F, G, H on the scale;

these several readings we called A, B, C, &c. An interpolation scale was then calculated and drawn, in which each division corresponded to a division on the scale of the instrument, and in which the points corresponding to the observations A, B, C, &c. were placed at the same distances apart as the same lines on our first drawings of the spectrum. By help of this scale, curves of the new spectra were drawn, in which the ordinates express the degrees of luminosity at the various points on the scale, as judged of by the eye. The lithographer then made the designs represented in fig. 4, Plate V. from these curves*.

As in our first memoir, so here we have represented only those lines which, in respect to position, definition, and intensity, serve as the best means of recognition. We feel it necessary to repeat this statement, because it has not unfrequently happened that the presence of lines which are not represented in our drawings has been considered as indicative of the existence of new bodies.

We have likewise added a representation of the potassium spectrum to those of the new metals for the sake of comparison, so that the close analogy which the spectra of the new alkaline metals bear to the potassium-spectrum may be at once seen. All three possess spectra which are continuous in the centre, and decreasing at each end in luminosity. In the case of potassium this continuous portion is most intense, in that of rubidium less intense, and in the cæsium-spectrum the luminosity is least. In all three we observe the most intense and characteristic lines towards both the red and blue ends of the spectrum.

Amongst the rubidium lines, those splendid ones named Rbα and Rbβ are extremely brilliant, and hence are most suited for the recognition of the metal. Less brilliant, but still very characteristic, are the lines Rbδ and Rbγ. From their position they are in a high degree remarkable, as they both fall beyond Fraunhofer's line A; and the outer one of them lies in an ultra-red portion of the solar spectrum, which can only be rendered visible by some special arrangement. The other lines, which are found on the continuous part of the spectrum, cannot so well be used as a means of detection, because they only appear when the substance is very pure, and when the luminosity is very great. Nitrate of rubidium, and the chloride, chlorate, and perchlorate of rubidium, on account of their easy volatility, show

* The coincidence of this chromolithograph plate with that published in the former memoir is by no means complete, but this does not seriously interfere with the utility of either of the representations; for if the position of an observed line be found, by help of the scale above described, to be near to that of the line of any known substance, it is easy, by placing some of this substance in one of the flames, and in the other some of the body under examination, to see whether the lines are coincident or not.

these lines most distinctly. Sulphate of rubidium and similar salts also give very beautiful spectra. Even silicate and phosphate of rubidium yield spectra in which all the details are plainly seen.

The spectrum of cæsium is especially characterized by the two blue lines Csα and Csβ; these lines are situated close to the blue strontia line Srδ, and are remarkable for their wonderful brilliancy and sharp definition. The line Csδ, which cannot be so conveniently used, must also be mentioned. The yellow and green lines represented on the figure, which first appear when the luminosity is great, cannot so well be employed for the purpose of detecting small quantities of the cæsium compounds; but they may be made use of with advantage as a test of the purity of the cæsium salt under examination. They appear much more distinctly than do the yellow and green lines in the potassium-spectrum, which, for this reason, we have not represented.

As regards *distinctness* of the reaction, the cæsium compounds resemble in every respect the corresponding rubidium salts: the chlorate, phosphate, and silicate gave the lines perfectly clearly. The *delicacy* of the reaction, however, in the case of the cæsium compounds is somewhat greater than in that of the corresponding compounds of rubidium. In a drop of water weighing 4 milligrammes, and containing only 0·0002 milligramme of chloride of rubidium, the lines Rbα and Rbβ can only just be distinguished; whilst 0·00005 milligramme of the chloride of cæsium can, under similar circumstances, easily be recognized by means of the lines Csα and Csβ.

If other members of the group of the alkaline metals occur together with cæsium and rubidium, the delicacy of the reaction is of course materially impaired, as is seen from the following experiments, in which the mixed chlorides contained in a drop of water weighing about 4 milligrammes, were brought into the flame on a platinum wire.

When 0·003 milligramme of chloride of cæsium was mixed with from 300 to 400 times its weight of the chlorides of potassium or sodium, it could be easily detected. Chloride of rubidium, on the other hand, could be detected with difficulty when the quantity of chloride of potassium or chloride of sodium amounted to 100 to 150 times its weight of the chloride of rubidium employed.

0·001 milligramme of chloride of cæsium was easily recognized when it was mixed with 1500 times its weight of chloride of lithium; whilst 0·001 milligramme of chloride of rubidium could not be recognized when the quantity of chloride of lithium added exceeded 600 times the weight of the rubidium salt.

At the close of this memoir we cannot refrain from touching upon a question to which, on some future occasion, we must

again recur. Amongst the large number of those salts already examined by us, which, owing to their volatility in the flame, render a spectrum-analysis possible, we have not found, in spite of the great variation in the elementary bodies combined with the metal, a single one which failed to produce the characteristic bright lines of the metal. Considering these numerous observations, made under the most widely differing circumstances, we might be led to suppose that in *all cases* the bright lines given out by a body occur quite independently of the other elements chemically combined with that body, and that therefore the relation of the elements, as regards the spectra of their vapours, is exactly the same, whether they are free or chemically combined. Yet this supposition is by no means founded on fact. We have repeatedly insisted that the bright lines in the spectrum of a luminous gas must coincide with the absorption lines which this gas produces in a continuous spectrum of a sufficient degree of luminosity. It is well known that the absorption lines of iodine vapour cannot be produced by hydriodic acid, and that, on the other hand, the absorption lines of nitrous acid are not visible in a mechanical mixture of nitrogen and oxygen. There is nothing to show why an influence of chemical combination upon the absorption lines, similar to that here noticed at low temperatures, should not occur at a white heat. If, however, the state of chemical combination alters the absorption lines of a luminous gas, it *must* likewise alter the bright lines of its spectrum.

From these considerations one would conclude that in the case of the spectra of two different compounds of the same metal, different bright lines may appear; it is, however, possible that the salts which are volatilized in the flame cannot exist at the temperature of the flame, and are decomposed, so that it may be in reality the vapour of the free metal which produces the lines; and it would then appear quite possible that a chemical compound may produce bright lines differing from those produced by its constituent elements.

LXV. *On the* Rev. T. P. Kirkman's *Problem respecting certain Triadic Arrangements of Fifteen Symbols. By* W. S. B. Woolhouse, *F.R.A.S., F.I.A., F.S.S. &c.**

IN the 'Lady's and Gentleman's Diary' for the year 1844, I proposed the following mathematical prize question:—

* Communicated by the Author.

Determine the number of combinations that can be made out of n symbols, p symbols in each; with this limitation, that no combination of q symbols which may appear in any one of them shall be repeated in any other.

This question, which essentially involves a developed theory of partitions, is more difficult than would at first appear; and it has not yet received anything like an approach to a complete general investigation, although it has given rise to some able papers on cognate subjects by Professor Sylvester, Mr. Cayley, &c. in the Philosophical Magazine and other scientific journals. The Rev. T. P. Kirkman, who, like Professor Sylvester, has gone somewhat elaborately into the subject of partitions, and has brought considerable ingenuity to bear upon his researches, has made the largest contributions towards the solution of the problem referred to. His early investigations in this particular field of inquiry led him to construct the following curious triadic problem, which was proposed amongst the queries given in the 'Lady's and Gentleman's Diary' for 1850:—

Fifteen young ladies in a school walk out three abreast for seven days in succession: it is required to arrange them daily so that no two shall walk twice abreast.

Two solutions, one of them by the talented proposer, were printed in the 'Diary' for 1851; but in these the results only were exhibited. Since that time the question has found its way into general society, and become somewhat noted as a fashionable puzzle, while, more scientifically considered, it has not failed to attract the attention of several eminent mathematicians.

Professor Sylvester, at the end of his paper "On a Four-valued Function," printed in the Philosophical Magazine for June last, page 520, has made some passing allusions to those mathematicians who, in common with himself, have contributed to the subject under consideration. On a recent perusal of this interesting paper I could not help noticing the summary character of these allusions, which first suggested to my mind the propriety of making the present communication with the view of pointing out the fact that the Rev. T. P. Kirkman originated this particular problem, and that it first appeared in the 'Diary.' I have at the same time been induced to give a systematic and comprehensive investigation of everything relating to it, in the 'Diary' for 1862, just published. In the investigation there given, it is shown that every solution to the problem must be contained in one or other of the three following systems:—

	1.	2.	3.	4.	5.	6.	7.
System No. 1.	$a_1 b_1 c_1$ $a_2 b_2 c_2$ $a_3 b_3 c_3$ $a_4 b_4 c_4$ $a_5 b_5 c_5$	$a_1 b_2 b_4$ $b_3 a_2 c_4$ $b_5 c_2 a_4$ $c_1 a_3 a_5$ $b_1 c_5 c_3$	$a_1 a_2 a_3$ $c_2 b_3 a_5$ $c_5 c_4 c_1$ $b_4 b_5 b_1$ $b_2 c_3 a_4$	$a_1 b_3 b_5$ $c_4 c_2 b_1$ $c_3 a_5 b_4$ $a_3 c_5 b_2$ $a_2 a_4 c_1$	$a_1 c_2 c_5$ $a_5 c_4 b_2$ $a_4 b_1 a_3$ $b_5 c_3 a_2$ $b_3 c_1 b_4$	$a_1 c_4 c_3$ $b_1 a_5 a_2$ $c_1 b_2 b_5$ $c_5 a_4 b_3$ $c_2 b_4 a_3$	$a_1 a_5 a_4$ $b_2 b_1 b_3$ $b_4 a_2 c_5$ $c_3 c_1 c_2$ $c_4 a_3 b_5$
System No. 2.	$b_1 a_1 c_1$ $a_2 c_2 b_2$ $a_3 b_3 c_3$ $b_4 c_4 a_4$ $c_5 a_5 b_5$	$b_1 a_2 a_5$ $c_2 a_3 b_4$ $b_2 b_5 c_1$ $c_5 a_4 b_3$ $a_1 c_4 c_3$	$b_1 c_2 c_4$ $a_3 b_2 c_5$ $b_4 c_3 a_5$ $a_1 b_3 b_5$ $a_2 a_4 c_1$	$b_1 a_3 a_4$ $b_2 b_4 a_1$ $c_5 c_1 c_4$ $a_2 b_5 c_3$ $c_2 b_3 a_5$	$b_1 b_2 b_3$ $b_4 c_5 a_2$ $a_1 a_5 a_4$ $c_2 c_3 c_1$ $a_3 b_5 c_4$	$b_1 b_4 b_5$ $c_5 a_1 c_2$ $a_2 c_4 b_3$ $a_3 c_1 a_5$ $b_2 c_3 a_4$	$b_1 c_5 c_3$ $a_1 a_2 a_3$ $c_2 a_4 b_5$ $b_2 a_5 c_4$ $b_4 c_1 b_3$
System No. 3.	$a_1 b_1 c_1$ $a_2 b_2 c_2$ $a_3 b_3 c_3$ $a_4 b_4 c_4$ $a_5 b_5 c_5$	$a_1 a_2 b_3$ $b_2 a_3 a_4$ $c_2 b_5 c_4$ $a_5 c_3 c_1$ $b_1 c_5 b_4$	$a_1 b_2 b_5$ $a_3 c_2 a_5$ $a_4 c_5 c_1$ $b_1 c_4 b_3$ $a_2 b_4 c_3$	$a_1 a_3 c_5$ $c_2 a_4 b_1$ $a_5 b_4 b_3$ $a_2 c_1 b_5$ $b_2 c_3 c_4$	$a_1 c_2 b_4$ $a_4 a_5 a_2$ $b_1 c_3 b_5$ $b_2 b_3 c_5$ $a_3 c_4 c_1$	$a_1 a_4 c_3$ $a_5 b_1 b_2$ $a_2 c_4 c_5$ $a_3 b_5 b_4$ $c_2 c_1 b_3$	$a_1 a_5 c_4$ $b\ a_2 a_3$ $b_2 c_1 b_4$ $c_2 c_5 c_3$ $a_4 b_3 b_5$

Here each of the seven combinations is derived from that which immediately precedes it by a fixed law of succession, which on continuation will circulate through exactly the same positions. Also the symbols occupying corresponding places, taken in order horizontally in each system, proceed according to two fixed cyclical series, each consisting of seven symbols, viz.—

$$\text{No. 1.} \begin{cases} b_1\ b_2\ a_2\ b_3\ c_2\ c_4\ a_5, \\ c_1\ b_4\ a_3\ b_5\ c_5\ c_3\ a_4; \end{cases}$$

$$\text{No. 2.} \begin{cases} a_1\ a_2\ c_2\ a_3\ b_2\ b_4\ c_5, \\ c_1\ a_5\ c_4\ a_4\ b_3\ b_5\ c_3; \end{cases}$$

$$\text{No. 3.} \begin{cases} b_1\ a_2\ b_2\ a_3\ c_2\ a_4\ a_5, \\ c_1\ b_3\ b_5\ c_5\ b_4\ c_3\ c_4. \end{cases}$$

The three systems are essentially independent of each other, and cannot be mutually transformed by symbolic substitution. The particular distribution of the primary combination of No. 2 has been modified, so as to exhibit the remarkable fact that the systems No. 1 and No. 2 admit of being made up of the same set of thirty-five triads, and so as to comprise the same triads in four of the seven combinations. Thus the 1st, 4th, 6th, and 7th combinations of No. 1 correspond to the 1st, 3rd, 2nd, and 5th combinations of No. 2. The thirty-five triads of No. 1 or No. 2 being collected, may be symmetrically arranged thus:—

$$a_1b_1c_1+a_2b_2c_2+a_3b_3c_3+a_4b_4c_4+a_5b_5c_5$$

$$+a_1\left|\begin{matrix}a_2a_3+b_1\\ a_4a_5\\ b_2b_4\\ b_3b_5\\ c_2c_5\\ c_3c_4\end{matrix}\right|\left|\begin{matrix}b_2b_3+c_1\\ b_4b_5\\ c_2c_4\\ c_3c_5\\ a_2a_5\\ a_3a_4\end{matrix}\right|\left|\begin{matrix}c_2c_3+a_2\\ c_4c_5\\ a_2a_4\\ a_3a_5\\ b_2b_5\\ b_3b_4\end{matrix}\right|\left|\begin{matrix}b_3c_4+b_2\\ b_4c_5\\ b_5c_3\end{matrix}\right|\left|\begin{matrix}c_3a_4+c_2\\ c_4a_5\\ c_5a_3\end{matrix}\right|\left|\begin{matrix}a_3b_4\\ a_4b_5\\ a_5b_3\end{matrix}\right|$$

$$+a_5b_4c_3+b_5c_4a_3+c_5a_4b_3;$$

and it is a still more remarkable fact that from this same set of triads it is possible to construct no less than 1080 systematic arrangements according to the first and second systems, each of them fulfilling the conditions of the problem.

The triads contained in the system No. 3 are essentially unsymmetrical, and admit of only one systematic arrangement.

In the 'Diary' I have also briefly indicated a direct and effectual method of systematizing any given solution when presented under an irregular form, and of thereby ascertaining to which of the three systems it belongs. As an example of the application of this method, the numerical solution given in the 'Diary' for 1851 is here transcribed for comparison with the same after being so adjusted and arranged*.

Solution by Mr. Samuel Bills, of Hawton, near Newark-upon-Trent; Mr. Thomas Jones, Abbey Buildings, Chester; Mr. Thomas Wainman, Burley, near Leeds; and Mr. W. H. Levy, of Shalbourne, near Hungerford.

1st day.	2nd day.	3rd day.	4th day.	5th day.	6th day.	7th day.
1 2 3	1 4 5	1 6 7	1 8 9	1 10 11	1 12 13	1 14 15
4 8 12	2 8 10	2 12 14	2 13 15	2 4 6	2 5 7	2 9 11
5 11 14	3 12 15	3 8 11	3 5 6	3 13 14	3 9 10	3 4 7
6 9 15	6 11 13	4 9 13	4 10 14	5 9 12	4 11 15	5 8 13
7 10 13	7 9 14	5 10 15	7 11 12	7 8 15	6 8 14	6 10 12

The same duly arranged: System No. 1.

5	14	11	5	10	15	5	13	8	5	4	1	5	7	2	5	9	12	5	3	6
13	10	7	4	13	9	7	4	3	9	7	14	3	9	10	14	3	13	10	14	4
8	4	12	1	7	6	2	9	11	12	3	15	6	14	8	11	10	1	15	13	2
6	15	9	11	8	3	15	1	14	8	2	10	1	12	13	2	6	4	12	11	7
3	1	2	14	2	12	10	12	6	13	6	11	4	11	15	7	15	8	9	8	1

I have further to observe that in constructing the first system the seven resulting combinations will be essentially the same, though occurring in a different order, when the primary combi-

* If any correspondents should think it worth while to communicate other solutions, I shall willingly systematize them in like manner.

nation is successively put under the following twenty-four forms, which for greater simplicity are here represented by numerals:—

A.			B.			C.			D.			α.			β.			γ.			δ.		
1	2	3	1	2	3	1	2	3	1	2	3	1	3	2	1	3	2	1	3	2	1	3	2
4	5	6	7	8	9	11	12	10	15	13	14	4	6	5	7	9	8	11	10	12	15	14	13
7	8	9	4	5	6	5	6	4	6	4	5	7	9	8	4	6	5	5	4	6	6	5	4
10	11	12	13	14	15	8	9	7	12	10	11	13	15	14	10	12	11	14	13	15	9	8	7
13	14	15	10	11	12	14	15	13	9	7	8	10	12	11	13	15	14	8	7	9	12	11	10

A′.			B′.			C′.			D′.			α′.			β′.			γ′			δ′.		
1	2	3	1	2	3	1	2	3	1	2	3	1	3	2	1	3	2	1	3	2	1	3	2
6	4	5	9	7	8	10	11	12	14	15	13	5	4	6	8	7	9	12	11	10	13	15	14
15	13	14	12	10	11	13	14	15	8	9	7	11	10	12	14	13	15	9	8	7	10	12	11
9	7	8	6	4	5	4	5	6	5	6	4	8	7	9	5	4	6	6	5	4	4	6	5
12	10	11	15	13	14	7	8	9	11	12	10	14	13	15	11	10	12	15	14	13	7	9	8

A″.			B″.			C″.			D″.			α″.			β″.			γ″.			δ″.		
1	2	3	1	2	3	1	2	3	1	2	3	1	3	2	1	3	2	1	3	2	1	3	2
5	6	4	8	9	7	12	10	11	13	14	15	6	5	4	9	8	7	10	12	11	14	13	15
11	12	10	14	15	13	9	7	8	10	11	12	15	14	13	12	11	10	13	15	14	8	7	9
14	15	13	11	12	10	15	13	14	7	8	9	12	11	10	15	14	13	7	9	8	11	10	12
8	9	7	5	6	4	6	4	5	4	5	6	9	8	7	6	5	4	4	6	5	5	4	6

In consequence of this flexibility in the disposition of the constituent triads of each combination, a solution obtained by a tentative process is most likely to belong to the first system. The seven combinations which result from the primaries A′, A″ follow in the order of those from A when the latter are taken with strides of two and four respectively; and so of the others.

To determine the number of synthetic combinations of the fifteen symbols that can be formed out of a given set of thirty-five triads, suppose pqr to be a triad taken as one of a combination: it can be associated only with the sixteen of the remaining triads that do not contain p, q or r. Let $p'q'r'$, taken from these, be the second triad; then $p'q'r'$ can be associated only with the six of the sixteen triads that do not contain p', q' or r'. Again, let a third triad $p''q''r''$ be taken from these; then $p''q''r''$ can be associated only with the two of the six triads that do not contain p'', q'' or r''; and these last will be the fourth and fifth triads of the combination. The number of combinations that can thus be made, comprising the fifteen symbols, will therefore be $35 \times 16 \times 6 \times 2$; and as the results will comprehend every form of permutation of the five triads under each combination, the total number of such combinations that can be formed, without permutation, will be $\frac{35 \times 16 \times 6 \times 2}{5 \times 4 \times 3 \times 2} = 56$. These combinations are stated at length in the 'Diary.'

Before concluding, I may be permitted to take the opportunity of briefly stating another analogous problem that may possibly interest some of the numerous readers of the Journal:—

> Sixteen symbols may be arranged five times in the form of a square, so that every pair of symbols shall appear once both in a horizontal and a vertical line.

If not hereafter anticipated, I may take a future opportunity of communicating a discussion of this neat problem.

Alwyne Lodge, Canonbury,
November 8, 1861.

LXVI. *Chemical Notices from Foreign Journals.* *By* E. Atkinson, *Ph.D., F.C.S.*

[Continued from p. 309.]

THERE are certain substances existing abundantly in nature, such as hydrogen, fluoride of silicon, and carbonic acid, which, without becoming fixed on the substances with which they come in contact, change them into mineral substances identical with those occurring in nature. These agents Deville* calls mineralizing agents, and in a series of experiments has shown that hydrochloric acid constitutes one. If sesquioxide of iron be heated to dull redness in a porcelain tube, and a rapid current of hydrochloric acid passed through it, sesquichloride of iron is condensed on the cooler parts of the apparatus, and water escapes along with the excess of acid. But if the current is slow and regular, the sesquioxide is changed into crystals quite identical in form with those of specular iron ore; at the same time as much hydrochloric acid escapes as enters the apparatus, not a trace of water being formed. If the temperature is very high, the crystals have the same form and the same angles as those of Elba iron ore; while if the temperature is lower, the crystals resemble volcanic specular iron ore.

Deville has also prepared† artificial cassiterite and rutile by the same method. Amorphous oxide of tin, obtained by the action of nitric acid on tin, is placed in a platinum tray which is heated in a porcelain tube to the fusing-point of copper, while a slow current of hydrochloric acid is transmitted through the tube. The oxide of tin remains behind in small but well-defined crystals, identical in form with those of the native mineral. When the current is rapid, a small quantity of bichloride of tin is formed, which is transported to the further end of the tube;

* *Comptes Rendus*, June 1861. † Ibid. July 22, 1861.

by an ulterior reaction of aqueous vapour this is changed into crystals which are somewhat larger and more perfect.

Hydrochloric acid also exercises a very curious reaction on amorphous titanic acid, changing it into very small crystals which resemble anatase or rutile. They are very lustrous, and have a bluish colour, like natural anatase. This blue colour arises from a partial reduction of titanic acid; for when this body was treated in a reducing atmosphere by gaseous hydrochloric acid, small crystals were obtained of a deep indigo-blue colour, the analysis of which showed that they were a new oxide of titanium,

$$Ti^3 O^5 = TiO^2, Ti^2 O^3.$$

Artificial rutile may also readily be obtained by the following method devised by Deville in conjunction with Caron. When a mixture of titanic acid and protoxide of tin is made, a titanate is formed at a red heat, which silica decomposes, forming a silicate and crystallized titanic acid. The operation is effected by heating to redness in an earthen crucible titanic acid along with oxide of tin and a little sand. A gangue is formed rich in tin, and in which are imbedded crystals of rutile 5 or 6 millims. in length. Their form is that of native rutile; they are pure and colourless if the materials are so, but they usually contain a trace of iron or manganese, which give them the colour of native rutile.

In the same manner Deville* has studied the formation of other minerals.

Magnetite.—Magnetic oxide of iron was prepared by heating protoxide of iron in a slow current of hydrochloric acid gas. Protochloride of iron was formed, but no water. The crystals remaining in the tray were regular octahedra, and were found on analysis to have the formula $Fe^3 O^4$. The protoxide of iron was prepared by Debray's method of heating sesquioxide of iron in a mixture of equal volumes of carbonic acid and carbonic oxide gases.

Periclase.—When calcined magnesia was heated in a slow current of hydrochloric acid gas, it was transformed without any loss into small crystals of periclase, which were sometimes white, sometimes greenish, and occasionally yellow from the presence of iron. Their form is the regular octahedron, and when produced at a very high temperature they are of considerable size. Chloride of magnesium in vapour is also decomposed by water, forming transparent octahedra of periclase.

Martite.—When a mixture of calcined magnesia and sesquioxide of iron was heated in a slow regular current of hydrochloric acid, two distinct substances were produced; one of them was

* *Comptes Rendus*, July 29, 1861.

periclase, and the other consisted of lustrous octahedra. They were found on analysis to have the composition $Fe^2 O^3 MgO$, and are true spinelles.

Hausmannite.—By heating red oxide of manganese in hydrochloric acid gas, small dimetric octahedra of Hausmannite, $Mn^2 O^3 MnO$, were obtained.

Protoxide of Manganese.—This was prepared by reducing any oxide of manganese in hydrogen, and heating it in the apparatus with a little hydrogen and a few bubbles of hydrochloric acid gas succeeding each other at long intervals. The small quantity of the latter gas required is truly surprising, and it escapes from the apparatus unaltered. The crystals obtained have a remarkable lustre; their colour is emerald-green, and they appear to be highly refringent, but exercise no action on polarized light. Their form is that of the cube-octahedron.

The same chemist has made the following observations*. When fluoride of silicon is passed over oxide of zinc at a high temperature, a mixture is formed of silicate and fluoride of zinc, which dissolve each other. The latter being volatile, on being heated the silicate is left in hexagonal prisms large enough to be readily measured, by which, and by their analysis, they were identified with native Willemite, $3ZnO, SiO^3$.

Fluoride of zinc acting upon silica gave the same products; so that a small quantity of fluoride of silicon could mineralize an indefinite quantity of silica and oxide of zinc.

Daubrée had stated that he had obtained Willemite and zircon by the action of chloride of silicon upon oxide of zinc or of zirconia. Deville, who has repeated these experiments with care, has found that neither Willemite nor zircon is formed; in fact by passing chloride of zircon over Willemite this substance is destroyed. This result might be expected; for the chlorides of silicon, in acting upon mineral oxides, do so not only by their chlorine, but by the metalloid, which exerts a powerful reducing action; and as the metallic chlorides formed under the influence of the chloride of silicon never dissolve the silicates formed, there is no reason why they should crystallize. The reverse is the case with fluoride of silicon; it is to the solvent effects of this substance on the silicates that its mineralizing properties are due. A series of experiments on the action of chloride of silicon on various metallic oxides, and which led to negative results, proved the correctness of these views.

Schiel has communicated† a determination of the atomic weight of silicon.

* *Comptes Rendus*, June 1861.

† Liebig's *Annalen*, October 1861.

A small glass bulb containing chloride of silicon was placed in water containing some ammonia, and the bulb broken. After the decomposition, the liquid was still feebly alkaline; it was allowed to stand for some days, then heated to boiling, filtered, and the silicic acid well washed out. The chlorine was precipitated from the filtrate by adding solution of nitrate of silver acidulated with nitric acid, and the chloride of silver washed out with water containing nitric acid. In two determinations 0·6738 and 1·3092 grm. of chloride of silicon gave respectively 2·277 and 4·418 of chloride of silver, from which is obtained the average 28·01 as the equivalent of silicon. The same number also follows from the vapour-density of chloride of silicon (5·39) and of fluoride of silicon (3·57), inasmuch as only the formulæ $SiCl^4$ and SiF^4 correspond to the vapour-density. Hence silicon, like tin and titanium, is quadratomic.

M. Rosensthiehl* has investigated the action of anhydrous sulphuric acid upon common salt. To a quantity of the former substance, placed in a stoppered retort, a quantity of powdered fused chloride of sodium was added. As the anhydrous acid generally contained a trace of the hydrated acid, sufficient heat was disengaged by the reaction thereby set up to melt the anhydrous acid. When the first reaction was over, the mixture was distilled until the residue fused. The distillate was analysed, and was found to be monochlorinated sulphuric acid formed in virtue of the reaction,

$$\underset{\text{Anhydrous sulphuric acid.}}{NaCl + 2S^2O^6} = \underset{\text{Bisulphate of soda.}}{NaO\,2S\,O^3} + \underset{\text{Monochlorinated sulphuric acid.}}{S^2O^5Cl}$$

A determination of the vapour-density confirmed the accuracy of this formula.

The new body is an oily colourless liquid of the spec. grav. 1·762, boiling between 145° and 150°, fuming in the air rather less than anhydrous sulphuric acid.

Projected upon a crystal of chromate of potash, it forms immediately chloride of chromyle,

$$KO\,CrO^3 + S^2O^5Cl = KO\,S^2O^6 + CrO^2Cl.$$

It acts in the cold on anhydrous acetate of soda without carbonizing it, and forms chloride of acetyle. This proves that it is a good chlorinizing agent; and as it is easily prepared, it may sometimes replace chloride of phosphorus, over which it has the advantage of not disengaging noxious vapours.

Wilson has described† a method for determining the hardness

* *Comptes Rendus*, October 7, 1861.

† Liebig's *Annalen*, September 1861.

of water, which is a modification of Clark's original method. He uses a solution of sulphate of lime prepared by dissolving 1 part $CaO\,SO^3, 2HO$ in 2543 parts of water. This corresponds to Clark's standard solution of 16 parts $CaO\,CO^2$ in 70,000 parts of water.

The solution of soap was prepared according to Faisst's method of dissolving 30 grms. of soda oil-soap in alcohol of 56° F., and diluting this solution so that 32 cubic centims. were exactly enough to produce, when shaken with 100 cubic centims. of the normal gypsum solution, a froth which remained for five minutes. By adding 4 cubic centims. of a cold saturated solution of carbonate of soda the reaction is made more regular, inasmuch as it changes all lime compounds into the carbonate which remains dissolved.

The experiments were made in the following manner:—By adding to the normal gypsum solution of 16 degrees hardness corresponding quantities of distilled water, sixteen solutions were prepared of from 1 to 16 degrees hardness. Of these solutions 100 cubic centims. were placed in a stoppered glass cylinder of 400 cubic centims. capacity, with 4 cubic centims. of the cold saturated solution of carbonate of soda, and solution of soap added from a burette until, on agitation, a light froth was formed. The solution was then added very gradually with continual agitation, until after the addition of the last drop a froth was formed lasting five minutes. The experimental results showed that the use of every 2 cubic centims. of solution of soap corresponds to 1 degree of hardness.

In order to test the hardness of water, 100 cubic centims. are measured off, 4 cubic centims. of saturated solution of carbonate of soda added until a froth is formed which remains standing five minutes. The number of cubic centims. of soap-solution divided by 2, gives the corresponding degree of hardness.

This method is not applicable to waters of more than 16 degrees hardness. With water of 20 degrees hardness, a precipitate of carbonate of lime is formed on the addition of carbonate of soda. Such waters must be diluted to a proper extent by the addition of distilled water.

In order to ascertain whether salts of magnesia exhibit the same deportment, a solution of 1 part sulphate of magnesia ($MgO\,SO^3+7HO$) was made in 1778 parts of water, which corresponds to a gypsum solution of 16 degrees hardness. By corresponding dilution with distilled water, solutions of 1, 4, 8, 12 degrees hardness were obtained, and these were estimated, after the addition of carbonate of soda, by means of soap-solution. The same results were obtained as with solutions of lime.

Wilson found, as Faisst had previously done, that mixtures of

lime and magnesia salts do not require less solution of soap, as stated by Dugald Campbell*. Liebig states in a note to Wilson's paper, that a determination made by Fink of the water of the chemical laboratory corresponded to 16 degrees hardness. A determination of the quantity of alkaline earths contained in a litre of water, gave 0·114 grm. lime and 0·035 grm. magnesia, which together correspond to 16 degrees of hardness.

Mendelejeff has investigated the expansion of liquids when heated above their boiling-point†. These experiments were made upon alcohol, ether, and water, and were effected by enclosing these liquids in glass tubes of about 300 millims. in length, 4 millims. internal diameter, and about 2 millims. thick in the glass. To free the liquid from air, some of the liquid was boiled in them, and they were then sealed. The upper part of the tube was divided into millimetres.

The tubes were heated by placing them vertically in a wide glass tube, and passing a current of the vapour of absolute alcohol, water, amylic alcohol, or oil of turpentine rapidly through the tube. The temperature was indicated by means of a thermometer placed along side the sealed tube. The observations were made by means of a cathetometer, and were read off when the height of the liquid in the tube and the corresponding temperature were constant. The vapours, after being used for heating, passed into a condensing apparatus. The experiments were made with all possible precautions, the details of which are described in the paper.

A very remarkable result was obtained from these experiments; that is, that the empirical formulæ of Kopp, which express the expansibility of ether, alcohol, and water up to the boiling-point, may also be applied with the same accuracy to the expansibility at far higher temperatures.

Mendelejeff also found that this held good for benzole and chloride of silicon, and concludes that the agreement of the calculated numbers with those obtained by experiment is so close that it will probably prevail with all liquids.

It further follows from these experiments that the expansion of liquids above the boiling-point obeys the same laws as under the boiling-point,—that the coefficient of expansion increases continually and gradually with diminution of the cohesion of the liquid, that is, with an increase of temperature. With some liquids it is as much as that of gas at a certain temperature; for example, for ether at 133°. The coefficient of expansion of ether amounts to 0·0054 at the temperature of its absolute boiling-

* Phil. Mag. vol. xxxvii. p. 171.
† Liebig's *Annalen*, July 1861.

point, that is, about 190°. We must consider that point to be the absolute boiling-point at which the cohesion of the liquid is equal to 0 and the coefficient of capillarity also 0, at which the latent heat of evaporation is also 0, and at which the liquid changes into vapour independently of pressure and volume. The absolute boiling-point of ether is at 190° (Wolff), that of chloride of silicon at 230° (Mendelejeff), of chloride of ethyle at 170°. For alcohol it must be about 250°, and for water 580°.

Corenwinder announced more than ten years ago that sulphur and hydrogen could be made to combine partially by bringing them in contact by means of pumice heated to redness. He has recently examined* the action of sulphur and aqueous vapour at high temperatures. In the middle of a glass or porcelain tube pieces of recently calcined pumice were placed, and near one end some pieces of sulphur, some asbestos being placed between the cork and the sulphur. The pumice was heated to redness and the sulphur slowly distilled over it, while at the same time a current of aqueous vapour was passed into the same end. After a little time sulphuretted hydrogen in abundance was produced. The phenomenon takes place in a still more distinct manner if pure calcined silica be substituted for pumice. In no case does the porous body undergo any alteration.

The author recommends this observation to the attention of geologists, for it explains the presence of sulphuretted hydrogen in certain gaseous exhalations which has been observed by MM. Boussingault, Ch. Deville, and Leblanc†.

It is well known that sulphurous acid is reduced, by the nascent hydrogen disengaged by means of zinc and hydrochloric acid, to sulphur and sulphuretted hydrogen. Kolbe has observed‡ that sulphuric acid is also similarly reduced. This he found by adding pure concentrated sulphuric acid through a funnel-tube to a mixture of water and zinc placed in a Woulfe's bottle. The gas is produced in larger quantities the hotter the mixture is which gives off hydrogen, and the more concentrated the sulphuric acid on coming in contact with the zinc.

When the sulphuric acid is previously diluted with double its volume of water, the disengaged hydrogen is quite free from sulphuretted hydrogen. If now concentrated sulphuric acid be added, there is a perceptible smell of sulphuretted hydrogen.

* *Comptes Rendus*, July 1861.
† Phil. Mag. vol. xvi. p. 284.
‡ Liebig's *Annalen*, August 1861.

LXVII. *On the Changes in the Induced Current by the employment of different Resistances.* By G. Magnus*.

THE remarkably great power of hydrogen to conduct heat which I had observed, induced me to compare the electrical conducting power of this gas with that of other gases. I encountered difficulties, however, which at last compelled me to believe that under certain hitherto unobserved circumstances opposite currents are formed, and that by means of these the irregularities of the deflection of the magnetic needle which I noticed were brought about. It was therefore necessary to investigate the conditions under which these currents were originated.

Poggendorff has shown† that if in the wire that completes the circuit of an induction apparatus in which there is an "electric egg," and in which only currents of a given direction are circulating, a Leyden jar be introduced, both polar wires of the "electric egg" are covered with blue light. Also the magnetic needle of a galvanometer placed in the current, though previously undergoing deflection, is now no longer turned aside, from which he inferred that by the introduction of the Leyden jar opposite currents were established. Since then the presence of blue light at the two poles of the "electric egg" has almost always been looked upon as the sign of the existence of opposite currents, and especially so, seeing that Riess‡ had even before this produced these appearances by opposite currents quickly following one another. It is certainly possible that blue light at both poles may not in all cases be an indubitable sign of the presence of opposite currents; still it is difficult to imagine that these appearances should have another cause. Nevertheless I shall not examine any further into this cause. To understand what follows, I emphatically remark that where the expression opposite currents is used, nothing more is signified than the presence of negative light at both polar wires. Dr. Paalzow§, in a research which appeared a short time ago "On the different ways of discharging the Leyden Battery, and on the Direction of Principal and Secondary Currents," used a similar phenomenon as a test. He, however, employed the so-called Geissler tubes, and observed them between the poles of an active electromagnet. I have made use of short tubes, 75 to 150 millims. in length, and 5 to 15 millims. in diameter, which were sealed after the air that they contained had been rarefied to from 4

* Translated from the *Sitzungsberichte der Akademie der Wissenschaften zu Berlin*, June 6, 1861.

† Poggendorff's *Annalen*, vol. xciv. p. 328.

‡ Ibid. vol. xci. p. 291. § Ibid. vol. cxii. p. 567.

to 6 millims. pressure by means of the air-pump. Attached to their platinum wires and melted with them into the glass, are aluminium wires, the points of which are from 6 to 40 millims. apart. If wires are used made of platinum only, the tubes very soon become covered on their interior surface with a black coating which makes them almost opake. This is not the case when aluminium is employed; hence Geissler* has used this metal in the preparation of his tubes for a long time. Such tubes, when made use of for investigating direction, I shall call test-tubes.

For the experiments, induced currents only were used. For this purpose two induction-apparatus were at my disposal, made by Ruhmkorff of Paris,—a smaller and older one, the dimensions of which may be considered as known, and a larger one, finished only a few months ago, the induction-wire of which has a length of 40,000 metres, and (without taking into account the silk with which it is covered) a diameter of 0·13 of a millim.

For both instruments a battery of two of Bunsen's elements was employed, with which the large coil furnished in the open air a spark of from 3 to 4 centims. in length. If it were set in action by a large battery, a spark was obtained which had a length of 39 centims. The apparatus, however, could not be employed of such a strength for the experiments to be mentioned presently.

Besides the test-tube, another tube was used, in which were two platinum wires 1 millim. in thickness and rounded at the ends, which wires, by means of a stuffing-box, could be placed at any desired distance from one another. In order to rarefy the air in this tube, it was attached to the air-pump. It is therefore distinguished from the electric egg by its being narrower, as well as longer, and admitting of a greater removal of the wires. For the sake of distinction I shall call this tube the air-tube.

If this air-tube, together with the test-tube, be inserted in

* It has often been maintained that particles of platinum are thrown over from the negative to the positive wire. This seems to me to be without foundation in the case of induced currents; for if the discharge takes place through such a tube as has already been described, during a long time and always in the same direction, it becomes covered with a black coating only in that part of the tube where the negative wire is, whilst in the neighbourhood of the positive wire no deposit is perceived even after a much longer time. I believe, therefore, that the black coating originates thus:—the platinum of the negative wire is either volatilized or thrown away from it, but not exactly towards the positive wire. For if the tube contain aluminium wires which are so short that the negative light extends over a part of the platinum wire to which the aluminium is attached, then the black coating is only formed in the neighbourhood of the platinum which is entirely removed from the positive wire.

the circuit of the induced current of either of the two induction-apparatus, and if by a certain rarefaction and a certain separation of the ends of the two wires only single currents make their appearance, then opposite currents are always formed if the ends of the wires are separated to such an extent that the electricity no longer passes between them in a luminous line, but spreads itself out in a brush-like form. On a further separation of the wires, opposite currents in every case show themselves in the test-tube. Instead of moving the wires, the same result can be obtained by gradually increasing the density of the air in the tube; in this case also, as soon as the brush-like discharge commences in the tube, the currents begin to be in opposite directions.

I believe it must be inferred from this that an increase of resistance gives rise to the opposite currents; and I have therefore employed the resistance of liquid and solid conductors instead of that of air. For this purpose the air-tube was replaced by a glass tube 1 metre in length and 3 millims. in diameter, in which two platinum wires could be approached towards, or separated from one another at pleasure. If this tube be filled with a saline solution, even if it contained only 0·25 per cent. of sulphate of potash, and if the wires were 900 millims. apart, it was impossible to obtain opposite currents. When, however, the tube contained pure water, the result was like that obtained by employing the air-tube. By a certain removal of the wires, only single currents were produced, whereas by a further separation they were opposite. It is likewise possible to produce opposite currents by means of the resistance of metallic conductors; it only needs for this purpose (provided, in addition to the test-tube, no air- or water-tube is inserted) the spiral of the large induction-coil, 40,000 metres in length, as a means of resistance; for, on the production of currents by the small induction-apparatus, they then make their appearance with great distinctness.

Further, if the resistance is increased in other ways, negative light appears on both wires. If the sparks from the large induction-apparatus are allowed to traverse the air and then a test-tube is inserted into the conducting wire, as long as the spark traverses the air vigorously, negative light is seen at only one pole of the test-tube; but if the spark goes through the air with a hissing noise, negative light appears at both poles.

We likewise obtain, by the introduction of a thin plate of mica into the circuit, which latter, with the exception of the test-tube, consists of nothing but metallic conductors, negative light on both wires. The same effect is produced, as Poggendorff*

* Poggendorff's *Annalen*, vol. xciv. p. 326.

has already shown, when a Leyden jar is introduced into the circuit.

If, instead of introducing the test-tube into the induction wire, we attach it to one end of the same and conduct the other to the earth, we likewise obtain opposite currents; or, to explain myself more cautiously, negative light appears at both wires.

If we melt into a small tube, which contains very rarefied air, only one wire and then attach it to one end of the induction-coil whilst the other is in contact with the earth, we obtain, if the tube be freely suspended in air, a luminous appearance on the above wire, which appearance is always of negative light; the tube may be in connexion with either end of the induction-wire; or, supposing it to remain at the same end, the current may traverse the wire in either direction. The intensity of the light is increased if we approach the tube with a conductor from the outside.

In how peculiar a manner glass influences the discharge, is seen from the following observation. If the air-tube be introduced into the wire which completes the circuit, and if its wires be so far separated that only single currents circulate, whilst on their being further removed from each other opposite currents arise, the passage from one wire to the other ceases as soon as the tube, thus arranged, is grasped with the hand; whilst in the test-tube that also forms a part of the circuit, opposite currents are immediately perceived. At the same time we observe (if not always, it is frequently the case) that the electricity in the air-tube passes to the glass. This phenomenon likewise makes its appearance in the electric egg; it is necessary, however, as this instrument is much wider than the tubes, to make its whole circumference a conductor by means of a strip of tinfoil. When we remove either the hand or the tinfoil, some time usually elapses before the passage from one wire to another is re-established.

From what has been before cited it may be inferred, and experience conclusively confirms it, that if the distance of the polar wires in the tubes, filled with water or air, be so chosen that on the employment of the large induction-coil single currents are still formed, then on replacing the large apparatus by the small one, opposite currents make their appearance*. The resistance

* For both apparatus one and the same contact-breaker was always used, the one which Ruhmkorff makes for his large induction-apparatus. With it the breaking of the contact is effected by the separation of a platinum wire from amalgam, the separation being, on Neef's principle, brought about by a small special electro-magnet, whose magnetism is induced by a single Daniell's element.

The breaking of the contact by means of Neef's hammer, already mentioned, whereby a point separates from a plate, diminishes the strength of

of the air-tube is too great for the intensity of the current which this apparatus produces; consequently we also find that the discharge no longer takes place in the shape of a bright luminous line, but in a brush-like form.

Not only are opposite currents produced if the resistance be too great in proportion to the intensity of the current, but also if the same be too small in relation to the discharge.

If we choose such a separation of the polar wires in the air-tube that the employment of the small induction-apparatus produces single currents, and if we then exchange the above mentioned for the large coil, opposite currents are generated.

We can obtain a similar result with one and the same induction-apparatus. If we attach the two ends of the spiral of the large induction-apparatus to the test-tube, and likewise insert the air-tube, in which the air is rarefied as much as possible, we see, when the polar wires of the latter are brought sufficiently near together, that both are covered with intense negative light. Let these wires be separated from one another, and it will be found that the negative light on the positive wire will gradually decrease, and increase on the negative wire, until the positive wire is entirely devoid of light. Could the wires be separated sufficiently, we should again have opposite currents; but the tube is not long enough for this. This result is obtained, however, by gradually admitting air into the tube and thereby increasing the resistance.

The idea suggests itself that these opposite currents, formed under so trifling a resistance, may have their origin therein—that not only by the breaking, but also by the establishing of the circuit, a current may be induced. It is well known that Poggendorff* has shown that, if the ends of an induction-coil are connected by means of a metallic wire or a liquid which is a good conductor, induced currents are formed on completing as well as on

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the induced current considerably more than the hammer used by Ruhmkorff for his small apparatus does, which hammer by its own weight completes the circuit. The latter has, however, an irregular action. I hoped by altering it in different ways to obtain a more regular movement, and for this purpose I employed two plates of osmium-iridium in order to avoid the adhesion, but I obtained no more favourable result. The contact-breaker made for the large induction-coil is in any case preferable. It likewise possesses the advantage, that we can accelerate its working at pleasure by shortening the pendulum attached to it. It is not, however, completely regular in its motion.

A short time ago Riess constructed an apparatus by means of which the breaking of the contact was brought about by clockwork—one of Mälzel's metronomes. It is possible that it will have a more regular action in consequence.

* Poggendorff's *Annalen*, vol. xciv. p. 309.

breaking the circuit, which circulate alternately to and fro. Since then, Gassiot* has called attention to the fact that we obtain a luminous appearance in tubes prepared by his method, on establishing the primary current, provided we use ten or more elements for the production of this current.

It was probable, therefore, that, if a test-tube were inserted which contained only a short stratum of very rarefied air, on establishing the principal current, generated in this case by two of Bunsen's elements, an induced current would be formed. This was found to be the case; for if the current was established by dipping the platinum wire of the contact-breaker only once with the hand into the amalgam, a luminous appearance was obtained in the test-tube, which, however, was considerably weaker than that produced by breaking the circuit. The opposite currents, observed with a very trifling resistance, depend therefore partly on the induced current which originates on completing the circuit. Still, I believe them to depend only *partly* thereon; for the current generated by once completing the circuit and then breaking it, also produces negative light at both polar wires. It might certainly be affirmed that we could produce no single breaking of the contact, that there is closing and breaking in succession; still there remains the remarkable fact, that by a single breaking of the contact the luminous appearance in the test-tube was always the same, whether the separation was produced quickly, by withdrawing a platinum point from the amalgam, or slowly, by disconnecting two copper surfaces like those in the rheotrope.

The following observation likewise shows the probability of the production of opposite currents in the induction-wire by a single breaking of the circuit.

It has just been mentioned that, if we choose such a separation of the polar wires in the air-tube that the employment of the small induction-apparatus produces single currents, and if we then replace this apparatus by the large coil, opposite currents are obtained. If, whilst the small induction-coil is in action, we first of all observe in the well-rarefied air-tube the negative wire, it appears to be covered to a considerable length with bluish light, whilst the positive, on the other hand, is entirely non-luminous. If we then employ the large induction-apparatus, a much smaller part of the negative wire is blue, whereas now a portion of the positive wire shines with this colour; the wires comport themselves just in this way if the circuit be broken once. It can scarcely be assumed that by such a single breaking of the contact a re-establishment of the circuit takes place which generates as strong a current as that which arises by the regular closing of the circuit. If, therefore, it is

* Phil. Mag. vol. xvi. p. 307.

not yet proved that opposite currents are produced by sufficiently weak resistance, still at the very least it is highly probable that such is the case.

Moreover, both Dr. Feddersen* and Dr. Paalzow have found that when the Leyden battery is discharged, opposite currents make their appearance if the resistance be trifling.

We may therefore regard as proved that induced currents are only single with a certain amount of resistance. Let the resistance exceed a fixed limit, and they are opposite; let it likewise sink below another certain limit, and they are also opposite. These boundaries vary according to the intensity of the current.

On the Changes of Colour of Electric Light.

In the test-tubes that I have employed, the negative light, which in rarefied atmospheric air is generally of an intense blue, is almost white; and in like manner the light extending from the positive pole to the dark intervening space was white, though it is usually red. I have endeavoured to find out the cause of these variations of colour.

When a newly-made tube of the prescribed kind is used, the negative light is at first blue, and the space between the wires is filled with red light; but soon afterwards both become brighter. The space between the two wires becomes brown, and finally white, and in the same way the negative light becomes entirely whitish. When this change has once been effected, the colour in the hermetically-sealed tube remains unchanged. If, however, we use a tube which can be opened, and consequently can have its air renewed, the negative light is at first blue and the intervening space red, but immediately afterwards both become white again.

This change cannot depend on the union of oxygen with aluminium; for in nitrogen gas, which in this case would be left, the colour of the electric light is very similar to what it is in atmospheric air. The appearance is most like that of the electric light in carbonic acid or hydrogen; but as neither of these gases was present, it occurred to me that perhaps the aluminium, during its preparation, might have come in contact with some foreign substance (some greasy matter for instance), and that by this means the phenomenon was brought about. Two aluminium wires, which were cut out of rolled plate, were consequently purified by scraping as much as possible, and without being touched by the fingers were fused into the tube. Under these circumstances the wire retained the light unchanged from what it was at the first moment, that is, continually blue at the negative wire, and red in the intervening space.

* Poggendorff's *Annalen*, vol. cxii. p. 452.

After the supposition, that the change in colour arose from the presence of a foreign substance, had thus been confirmed, I found that in so narrow a tube even the smallest quantity of fatty matter on the negative wire was sufficient to make the light white. Mere contact with the fingers is often sufficient for this purpose; and, indeed, not only by the employment of wires of aluminium, but likewise of copper, brass, platinum, and probably every other metal which is not volatile at the temperature in question, the same results are obtained. On the positive wire fat produces little or no effect; it may either be placed on its point, or at some distance from it.

Tallow, fatty oils, stearic acid, and wax behave all in a similar way. When we place some of either of the above on the negative wire, the greasy part appears red at first, whilst the rest of the wire shines with blue light. Immediately afterwards the spot is surrounded with a mantle of reddish light which gradually disappears. The blue light on the rest of the wire simultaneously turns white, and the red light between the two wires likewise changes to brown and white. It is probable that the fat was decomposed, but through the quantity being so small the decomposition could not be proved.

LXVIII. *On the Measurement of the Intensity of Electric Currents by means of a Tangent-galvanometer or a Multiplier.* *By* Ch. V. Zenger, *Professor of Physics, and Member of the Physical Institute**.

MR. G. JOHNSTONE STONEY published in this Journal (February 1858) a formula of correction for the length of the needle and for the derangement of its point of suspension. Mr. Stoney in his paper has mentioned the researches of MM. Gaugain and Bosscha, but he does not notice my researches on the corrections to be applied to tangent-galvanometers and to multipliers, although I published them three years ago†, and although they were discussed in Liebig and Kopp's *Jahresbericht*. Mr. Johnstone Stoney examines two cases, viz. that of a common galvanometer, and that of Gaugain's arrangement; he found in the first case the formula of correction,

$$i = K \tan\theta \left\{ 1 + \frac{15}{4} \lambda^2 \sin^2\theta \right\},$$

K being a constant so long as the same needle is used, $\theta =$ the

* Communicated by the Author.

† Proceedings of the Imperial Academy of Vienna, vol. xvii. April 19; and vol. xviii. November 2, 1855.

angle of deflection from the magnetic meridian, and $2\lambda =$ the length of the needle.

Though this equation is very similar to my formula of correction, yet both are not identical, and do not afford the same approximation to real intensity. As M. Gaugain's arrangement considerably lessens the sensibility of tangent-galvanometers, and as it cannot be applied to the construction of multipliers, it may perhaps be of use and interest to call to mind the formula of correction which I gave in 1855.

Conceive a circular or elliptical band of metal to be placed in the plane of the magnetic meridian, together with a magnetic needle, the centre of which coincides with the centre of the band; imagine now the action of an element A of an electric current to be p at the distance 1, and p' at a distance δ; then

$$p' = pf(\delta),$$

and the total action of the current

$$\left.\begin{aligned} S &= p' + p'' + p''' + \ldots + p_n = p\{f(\delta) + f(\delta') + f(\delta'') + \ldots f\delta_n\}, \\ S &= pf(\delta)\left\{1 + \frac{f(\delta')}{f\delta} + \frac{f(\delta'')}{f(\delta)} + \ldots + \frac{f(\delta_n)}{f(\delta)}\right\} = pf(\delta)\Sigma f(\delta), \end{aligned}\right\} (1)$$

$\Sigma f'(\delta)$ being a constant as long as the needle does not deviate from the magnetic meridian. The action of two different currents on the same galvanometer would be

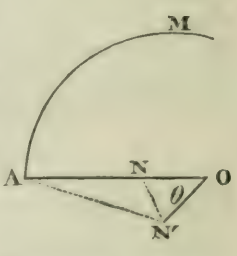
$$S : S' = pf(\delta)\Sigma f'(\delta) : p'f(\delta)\Sigma f'(\delta) = p : p',$$

if the needle remained in the plane of the band.

The poles of the needle being deviated, the distances $\delta \ldots \delta_n$ become increased, and the magnetic action of the current decreases at the rate

$$\left.\begin{aligned} S : S' &= AN'^2 : AN^2, \\ S : S' &= \frac{1 + 4\lambda(\delta + \lambda)}{\delta^2} \sin^2 \tfrac{1}{2}\theta : 1, \end{aligned}\right\} \quad . \quad . \quad (2)$$

θ being the angle of deviation from the magnetic meridian, $2\lambda =$ the length of the needle, and $\delta =$ the distance AN. We find $AN + NO = AO$, or $\delta + \lambda = a$, $2a$ being the axis of the circular or elliptic band, and



$$S' = \frac{S}{1 + \frac{4a\lambda}{(a-\lambda)^2} \sin^2 \frac{1}{2}\theta}.$$

The constant $c = \frac{4a\lambda}{(a-\lambda)^2}$ rapidly increases when a nearly equals λ;

it is equal to 1 when $\frac{a}{\lambda} = 5{\cdot}828426$, that is to say, when the length of the needle is nearly one-sixth the length of the axis a. It then rapidly decreases; and as it has to be multiplied with $\sin^2\frac{1}{2}\theta$, it exerts but little influence on the measurements by means of a tangent-galvanometer, the needle of which has usually only one-seventh or one-eighth the length of the axis a. The needle of multipliers being very long, the influence of the term $c\sin^2\frac{1}{2}\theta$ becomes increased in such a manner as to prevent the possibility of measuring according to the law of tangents. For that reason I proposed to use multipliers consisting of an elliptic metal band or coil of copper wire with a single needle (made astatic by means of a magnetic bar approached to it with the same pole); the needle may then be taken of any length, and it may be made perfectly astatic if required. The needle being very long, c becomes so increased that it can be omitted in the equation

$$S : S' = 1 + c\sin^2\tfrac{1}{2}\theta : 1 + c\sin^2\tfrac{1}{2}\theta' = \frac{1}{c} + \sin^2\tfrac{1}{2}\theta : \frac{1}{c} + \sin^2\tfrac{1}{2}\theta'$$

$$S : S' = \sin^2\tfrac{1}{2}\theta : \sin^2\tfrac{1}{2}\theta'.$$

We find, in conformity with the law of tangents,

$$S' = H\tan\theta \text{ and } S = H\tan\theta(1 + c\sin^2\tfrac{1}{2}\theta),$$

$$S : S' = (1 + c\sin^2\tfrac{1}{2}\theta)\tan\theta : (1 + c\sin^2\tfrac{1}{2}\theta')\tan\theta'.$$

The needle being very long, we find with sufficient approximation,

$$S : S' = \sin^2\tfrac{1}{2}\theta\tan\theta : \sin^2\tfrac{1}{2}\theta'\tan\theta'.$$

To convey a clear idea of the accuracy to be attained by the formula of Mr. Johnstone Stoney and mine, I applied each of them to a series of observations made by means of a tangent-galvanometer, the needle of which had (with regard to the diameter of the circular metal band of 202·5 millims.) the enormous length of 190 millims. I observed the intensity of currents of a constant electromotive apparatus when 0·1, 0·2, and ultimately the whole length of the metal plates was immersed.

Immersion.	Deflections.		Average.	Immersion.	Deflections.		Average.
	North pole.	South pole.			North pole.	South pole.	
0·1	15°0	195°5	15° 15′	0 6	25°2	205°4	25° 18′
0·2	17·2	197·5	17 21	0·7	26·2	206·7	26 41
0·3	20·0	200·4	20 12	0·8	27·5	208·0	27 45
0·4	22·5	202·5	22 39	0·9	28·5	209·0	28 45
0·5	24·0	204·5	24 15	1·0	29·8	210·2	29 54

Intensity of the compared currents.		$\tan\theta$. $\tan\theta'$	Formulæ of correction.		Errors committed, in per cents.	
			Stoney.	Zenger.	Stoney.	Zenger.
					per cent.	per cent.
1 : 2	0·50000	0·86029	0·67236	0·58750	34·5	11·8
1 : 3	0·33333	0·72750	0·38854	0·38273	16·6	14·8
1 : 4	0·25000	0·65335	0·31520	0·27075	26·1	8·3
1 : 5	0·20000	0·60523	0·25120	0·21994	25·6	10·0
1 : 6	0·16667	0·57677	0·22136	0·19319	32·8	15·9
1 : 7	0·14286	0·54208	0·18846	0·16365	28·1	14·6
1 : 8	0·12500	0·51819	0·16765	0·14522	34·1	16·2
1 : 9	0·11111	0·49695	0·15074	0·13010	35·7	17·1
1 : 10	0·10000	0·47412	0·13394	0·11482	33·9	14·8
				Average	29·69	: 13·72
					2·165	: 1

The errors arising from the use of Mr. Johnstone Stoney's formula considerably exceed those arising from the use of my own formula. No doubt these numbers prove the advantages of this formula, and they suffice to show distinctly that, in conducting investigations in which accuracy is a point of importance, Mr. Johnstone Stoney's formula cannot be used.

Vienna, January 8, 1862.

LXIX. *On the Circularity of the Sun's Disc.* *By* G. B. Airy, *Esq., Astronomer Royal**.

IT has been proposed lately to prepare an apparatus for the purpose of examining whether the Sun's disc is really circular, and in particular for ascertaining whether the diameters nearly perpendicular to the ecliptic are equal to those nearly parallel to the ecliptic. I would not by any means discourage the trial of such apparatus; but I would unhesitatingly express my opinion that the result of the trial would be to show whether the apparatus is or is not trustworthy, and not to give any new information regarding the measure of the Sun's diameters in any degree comparable to that which we already possess.

Perhaps few persons except professional astronomers are aware of the enormous amount of evidence which already exists in reference to the values of the Sun's diameters, and of the way in which this evidence is growing every day in the ordinary routine of meridional observations. To make this fully understood, I will here explain what is prepared in the Nautical Almanac, what is observed at the Royal Observatory, how the observations are

* From the Monthly Notices of the Royal Astronomical Society, January 10, 1862.

reduced, and how the comparison of the reduced observations with the numbers of the Nautical Almanac bears upon the subject now before us.

For the calculations of the Nautical Almanac, an assumption is made as to the numerical value of the Sun's diameter as seen when the Earth is at its mean distance from the Sun. It matters not whether this assumed diameter is or is not correct, provided that it be used consistently in all the calculations of each year; and it matters not whether it be or be not changed from year to year, provided that each volume contain a statement of the assumed diameter which has actually been used in the calculations of that volume. Thus the assumed value of Sun's diameter, as seen at Earth's mean distance, in the Nautical Almanacs from 1836 to 1852 was $32'\ 1''{\cdot}80$; that in the subsequent Nautical Almanacs is $32'\ 3''{\cdot}64$.

With the diameter thus assumed, two sets of numbers are computed in the Nautical Almanac. One is the apparent diameter (or semidiameter) of the Sun at noon on every day; this is found by merely altering the assumed diameter in the inverse proportion of the Earth's varying distance from the Sun. The other is the duration of passage of the Sun's diameter across the meridian, or the measure of the sidereal time which elapses between the passage of the Sun's western limb and its eastern limb; this is found from the apparent diameter of the day, by introducing the consideration of the Sun's declination and of the Sun's motion in right ascension. And these numbers being prepared, it is evident that we have elements which correspond very closely with facts that may be observed, the elements being essentially based on the supposition that the Sun's disc is circular.

Corresponding to these two classes of computed elements, we have two classes of facts observed at the Royal Observatory and at other observatories. One is the zenith distance of the Sun's upper limb and that of the Sun's lower limb. When each of these is corrected separately for refraction and parallax, the true results of geocentric observation are obtained; and the difference between them gives the observed *vertical* diameter of the Sun on the day of observation. The other is the sidereal time shown by the transit-clock at the instant of transit of the Sun's western limb, and that at the transit of the Sun's eastern limb; the difference between these gives the observed duration of passage of the Sun's *horizontal* diameter across the meridian on the day of observation.

Now if we compare each of these numbers separately (namely, the observed vertical diameter and the observed duration of passage of horizontal diameter) with the corresponding numbers in

the Nautical Almanac, and if we omit consideration of chance errors of observation, the effect of which may be supposed to be nearly eliminated in the mean of many observations, the following results ought to hold:—If the Sun's disc is really circular, and if the Nautical-Almanac assumed diameter at mean distance is correct, then the observed vertical diameter will agree with the Nautical-Almanac diameter for the day, and the observed duration of passage will agree with that of the Nautical Almanac. If the Sun's disc is really circular, but the assumed diameter incorrect, then neither of the compared measures will agree with the corresponding computation of the Nautical Almanac; each discordance (one of vertical diameter, the other of duration of passage of horizontal diameter) will indicate a numerical value of correction to be applied to the assumed diameter; but the two numerical values will absolutely agree. But if the Sun's disc is not really circular, then it is impossible that the comparison of observed vertical diameters on the one hand, and of observed durations of passage of horizontal diameters on the other hand, with elements computed on the supposition that the Sun is circular, can indicate the same correction to the assumed semidiameter.

All that is necessary, therefore, for ascertaining whether the Sun's horizontal diameter and the Sun's vertical diameter are equal, is every day to compare the Sun's observed vertical diameter with the Nautical-Almanac diameter, and the observed duration of passage of Sun's horizontal diameter with the Nautical-Almanac duration, and to infer separately from these the correction to be made to the Nautical-Almanac assumed diameter. If the two results agree, the horizontal and vertical diameters are equal.

Now these comparisons are made every day in the routine of the Royal Observatory; and their results will be found in one of the late sections of each volume of the printed 'Greenwich Observations,' as well as in the more extensively distributed 'Results of the Greenwich Observations,' which contain that section; and the means of the numbers for each year are given in the Introduction to each volume. By extracting these numbers, the following Table is formed. I have thought it necessary to divide the Table into three parts, distinguished by the following circumstances:—From 1836 to 1850 the 4-inch telescope (I believe Dollond's) of the Mural Circle was used for the vertical diameters, and the 5-inch telescope (Dollond's) of the Transit for the horizontal passages; the diameter used in the computations of the Nautical Almanac was $32'\ 1''{\cdot}80$. Through 1851 and 1852 the 8-inch telescope (Simms's) of the Transit Circle was used for both measures, the Nautical-Almanac assumed diameter

being still 32′ 1″·80. From 1853 to 1860 the telescope of the Transit-Circle was used for both measures, but the Nautical Almanac assumed diameter was 32′ 3″·64.

Apparent Errors of the Duration of Passage of the Sun's Horizontal Diameter, and of the Sun's Vertical Diameter, as computed in the Nautical Almanac.

Year.	No. of obs. of horizontal diameter.	Mean value of N.-A. Ob.	No. of obs. of vertical diameter.	Mean value of N.-A. Ob.
		s		″
1836.	104	−0·17	116	−1·50
1837.	92	−0·18	122	−1·85
1838.	108	−0·14	115	−1·53
1839.	103	−0·14	114	−0·94
1840.	104	−0·18	112	−1·60
1841.	102	−0·14	109	−1·32
1842.	116	−0·14	121	−1·54
1843.	99	−0·14	107	−1·84
1844.	102	−0·18	117	−1·50
1845.	100	−0·17	113	−1·32
1846.	92	−0·10	101	−2·09
1847.	89	−0·05	89	−2·98
1848.	94	−0·06	102	−2·06
1849.	103	−0·11	101	−2·40
1850.	94	−0·11	86	−2·18
1851.	87	−0·08	106	−0·71
1852.	103	−0·12	112	−1·39
1853.	78	0·00	86	+0·58
1854.	109	+0·09	111	+1·29
1855.	84	+0·07	93	+0·65
1856.	104	+0·10	109	+1·17
1857.	113	+0·07	123	+0·99
1858.	126	+0·07	132	+0·92
1859.	109	+0·08	125	+0·99
1860.	72	+0·09	72	+1·64

If we take the sums of the numbers of observations and the means of the errors, and if we remark that the mean error of the horizontal diameter in arc may be obtained from the mean error of the duration of passage without sensible error by multiplying by 14, we obtain the following numbers:—

Period.	No. of obs.	Mean error of N.-A. duration of passage.	Mean error of N.-A. horizontal diameter.	No. of obs.	Mean error of N.-A. vertical diameter.
		s	″		″
1836 to 1850.	1502	−0·134	−1·88	1625	−1·78
1851 and 1352.	190	−0·100	−1·40	218	−1·05
1853 to 1860.	795	+0·071	+0·99	851	+1·03

If we change the signs of these errors to form corrections, and

apply them to the assumed diameter at mean distance of the Earth from the Sun (namely, 32′ 1″·80 to the end of 1852, and 32′ 3″·64 from the beginning of 1853), to produce a corrected diameter of the Sun at mean distance, we form the following Table:—

Instruments employed.	Period.	No. of obs.	Corrected horizontal diameter.	No. of obs.	Corrected vertical diameter.
Transit and Mural Circle	1836 to 1850	1502	32′ 3″·68	1625	32′ 3″·58
Transit Circle	{1851 and 1852}	{190}	{32 3·20}	{218}	{32 2·85}
	{1853 to 1860}	{795}	{32 2·65}	{851}	{32 2·61}
	Mean 1851 to 1860	985	32 2·76	1069	32 2·66

Thus the observations with both classes of instruments, in aggregate number 2487 for horizontal diameter, and 2694 for vertical diameter, agree in showing that the horizontal diameter exceeds the vertical diameter by only 0″·1, a quantity smaller than we can answer for in these or in any other methods of observation.

A consideration of the number and excellence of the observations fully supports the view which I have stated in introducing this subject,—that the only result which could be deduced from the trial of new apparatus would be to test the apparatus, but not to add to the certainty of the conclusion as to the equality of diameters.

The diameter adopted now in the Nautical Almanac was inferred from observations made with the Transit and Mural Circle, and therefore agrees very closely with that here deduced from the use of those instruments. That obtained with the Transit Circle is less by 0″·93.

Royal Observatory, Greenwich,
December 28, 1861.

LXX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 485.]

January 10, 1861.—Major-General Sabine, R.A., Treasurer and Vice-President, in the Chair.

THE following communications were read:—

"On the Equation for the Product of the Differences of all but one of the Roots of a given Equation." By Arthur Cayley, Esq., F.R.S.

"Description of a new Optical Instrument called the 'Stereotrope'." By William Thomas Shaw, Esq.

This instrument is an application of the principle of the stereoscope to that class of instruments variously termed thaumatropes, phantascopes, phenakistoscopes, &c., which depend for their results on "persistence of vision." In these instruments, as is well known, an object represented on a revolving disc, in the successive positions it assumes in performing a given evolution, is seen to execute the movement so delineated; in the stereotrope the effect of solidity is superadded, so that the object is perceived as if in motion and with an appearance of relief as in nature. The following is the manner in which I adapt to this purpose the refracting form of the stereoscope.

Having procured eight stereoscopic pictures of an object—of a steam-engine for example—in the successive positions it assumes in completing a revolution, I affix them, in the order in which they were taken, to an octagonal drum, which revolves on a horizontal axis beneath an ordinary lenticular stereoscope and brings them one after another into view. Immediately beneath the lenses, and with its axis situated half an inch from the plane of sight, is fixed a solid cylinder, 4 inches in diameter, capable of being moved freely on its axis. This cylinder, which is called the eye-cylinder, is pierced throughout its entire length (if we except a diaphragm in the centre inserted for obvious reasons) by two apertures, of such a shape, and so situated relatively to each other, that a transverse section of the cylinder shows them as cones, with their apices pointing in opposite directions, and with their axes parallel to, and distant half an inch from, the diameter of the cylinder. Attached to the axis of the eye-cylinder is a pulley, exactly one-fourth the size of a similar pulley affixed to the axis of the picture-drum, with which it is connected by means of an endless band. The eye-cylinder thus making four revolutions to one of the picture-drum, it is evident that the axes of its apertures will respectively coincide with the plane of sight four times in one complete revolution of the instrument, and that, consequently, vision will be permitted eight times, or once for each picture.

The cylinder is so placed that at the time of vision the *large* ends of the apertures are next the eyes, the effect of which is that when the *small* ends pass the eyes, the axes of the apertures, by reason of their eccentricity, do not coincide with the plane of sight, and vision is therefore impossible. If, however, the position of the cylinder be reversed end for end, vision will be possible only when the small ends are next the eyes, and the angle of the aperture will be found to subtend exactly the pencil of rays coming from a picture, which is so placed as to be bisected at right angles by the plane of sight. Hence it follows that, the former arrangement of the cylinder being reverted to, the observer looking along the upper side of the aperture will see a narrow strip extending along the top of the picture; then, moving the cylinder on and looking along the lower side of the aperture, he will see a similar strip at the bottom of the picture; consequently, in the intermediate positions of the aperture, the other

parts of the picture will have been projected on the retinæ. The width of these strips is determined by that of the small ends of the apertures, which measure ·125 inch; and the diameter of the large ends is 1·5 inch, the lenses being distant 9 inches from the pictures. The picture-drum being caused to revolve with the requisite rapidity, the observer will see the steam-engine constantly before him, its position remaining unchanged in respect of space, but its parts will appear to be in motion, and in solid relief, as in the veritable object. The stationary appearance of the pictures, notwithstanding the fact of their being in rapid motion, is brought about by causing their corresponding parts to be seen, respectively, *only* in the same part of space, and *that* for so short a time that while in view they make no sensible progression. As, however, there is an actual progression during the instant of vision, it is needful to take that fact into account—in order that it may be reduced as far as practicable—in regulating the diameter of the eye-cylinder, and of the apertures at their small ends; and the following are the numerical data involved in the construction of an instrument with the relative proportions given above:—

The circumference of picture-drum=22·5 inches (A).

The circumference of eye-cylinder=12 inches×4 revolutions=48 inches (B).

The diameter of apertures at large ends=1·5 inch (C).

The diameter of apertures at small ends=·125 inch (D).

While the large end is passing the eye, the picture under view progresses $\frac{1\cdot5\ (C)}{48\ (B)}$ of 22·5 (A), or ·703 inch.

This amount of progression (·703 in.), if perceived at one and the same instant, would be utterly destructive of all distinctness of definition; but it is evident that the total movement brought under visual observation at any one moment is $\frac{\cdot125\ (D)}{1\cdot5\ (C)}$ of ·703 inch, or ·058 inch. This movement must necessarily occasion a corresponding slurring, so to speak, of the images on the retina; and the fact of such slurring not affecting, to an appreciable extent, the distinctness of definition, seems to be referable to a faculty which the mind has of correcting or disregarding certain discrepant appearances or irregularities in the organ of vision; as a further illustration of which I may cite the fact, mentioned by Mr. Warren De la Rue in his "Report on Celestial Photography," that the retinal image of a star is, at least under some atmospheric conditions, made up of "a great number of undulating points," which, however, the mind rightly interprets as the effect of the presence before the eye of a single minute object. That this corrective power is, as might be supposed, very limited, may be proved experimentally by this instrument; for if the small ends be enlarged in only a slight degree, so as to increase this slurring on the retinæ, a very marked diminution in clearness of definition is the immediate result.

That form of the stereotrope, in which Professor Wheatstone's reflecting stereoscope is made use of, and which is better adapted for

the exhibition of movements that are not only local but progressive in space, it is needless to describe here, because the principles it involves are essentially the same as those which are stated above.

Jan. 17.—"On the Homologies of the Eye and of its Parts in the Invertebrata." By J. Braxton Hicks, M.D. Lond., F.L.S.

Jan. 24.—"On the Calculus of Symbols, with Applications to the Theory of Differential Equations." By W. H. L. Russell, A.B.

January 31.—Major-General Sabine, R.A., Treasurer and Vice-President in the Chair.

The following communications were read:—

"On Systems of Linear Indeterminate Equations and Congruences." By H. J. Stephen Smith, Esq., M.A.

The present communication relates to the theory of the solutio in positive and negative integral numbers, of systems of linear indeterminate equations, having integral coefficients. In connexion with this theory, a solution is also given of certain problems relating to rectangular matrices, composed of integral numbers, which are of frequent use in the higher arithmetic. Of this kind are the two following:—

1. "Given (in integral numbers) the values of the determinants of any rectangular matrix of given dimensions, to find all the matrices, the constituents of which are integers, and the determinants of which have those given values.

2. "Given any rectangular matrix, the determinants of which have a given number D for their greatest common divisor, to find all the supplementary matrices, which, with the given matrix, form square matrices, of which the determinant is D."

A solution of particular, but still very important cases of these two problems, has been already given by M. Hermite. The method by which in this paper their general solution has been obtained, depends on an elementary, but apparently fertile principle in the theory of indeterminate linear systems; viz. that if m be the *index of indeterminateness* of such a system (*i. e.* the excess of the number of indeterminates above the number of really independent equations), it is always possible to assign a set of m solutions, such that the determinants of the matrix formed by them shall admit of no common divisor but unity.

Such a set of solutions is termed a *fundamental* set, and possesses the characteristic property, that every other solution of the system can be integrally expressed by means of the solutions contained in it. A set of *independent* solutions is one in which the determinants of the matrix have a finite common divisor, *i. e.* are not all zero. The theory of independent and fundamental sets of solutions in some respects resembles that of independent and fundamental systems of units in Lejeune Dirichlet's celebrated generalization of the solution of the Pellian equation.

By the aid of the same principle of fundamental sets, the follow-

ing criterion is obtained for the resolubility or irresolubility of indeterminate linear systems.

"A linear system is or is not resoluble in integral numbers, according as the greatest common divisor of the determinants of the matrix of the system is or is not equal to the corresponding greatest common divisor of its *augmented* matrix."

[The matrix of a linear system of equations is, of course, the rectangular matrix formed by the coefficients of the indeterminates; the *augmented* matrix is the matrix derived from that matrix, by adding to it a vertical column composed of the absolute terms of the equations.]

A system of linear congruences may, of course, be regarded as a system of linear indeterminate equations of a particular form; and the criterion for its resolubility or irresolubility is implicitly contained in that just given for any indeterminate system. But this criterion may be expressed in a form in which its relation to the modulus is very clearly seen.

Let

$$A_{i,1}x_1+A_{i,2}x_2+\ \ldots\ +A_{i,n}x_n\equiv A_{i,n+1},\ \mathrm{mod}\ M,\ i=1,\ 2,\ 3,\ \ldots\ n$$

represent a system of congruences; let us denote by ∇_n, ∇_{n-1}, $\ldots$ ∇_1, ∇_0, the greatest common divisors of the determinant, first minors, &c., of the matrix of the system [so that, in fact, ∇_n is the determinant itself, ∇_1 the greatest common divisor of the coefficients $A_{i,j}$, and $\nabla_0=1$]; by D_n, D_{n-1}, $\ldots$ D_1, D_0 the corresponding numbers for the *augmented* matrix; let also δ_i and d^i respectively represent the greatest common divisors of M with $\dfrac{\nabla_i}{\nabla_{i-1}}$, and of M with $\dfrac{D_i}{D_{i-1}}$; and put

$$m=d_n\times d_{n-1}\times\ \ldots\ \times d_1,$$

$$\mu=\delta_n\times\delta_{n-1}\times\ \ldots\ \times\delta_1,\ \ldots$$

Then the necessary and sufficient condition for the resolubility of the system is

$$m=\mu;$$

and when this condition is satisfied, the number of solutions is precisely m.

The demonstration of this result (which seems to exhaust the theory of these systems) is obtained by means of the following theorem:—

"If $\|A\|$ represent any square matrix in integral numbers, ∇_n its determinant, ∇_{n-1}, ∇_{n-2}, $\ldots$ ∇_1, ∇_0 the greatest common divisors of its successive orders of minors, it is always possible to assign two unit-matrices $\|\alpha\|$ and $\|\beta\|$, of the same dimensions as $\|A\|$, and satisfying the equation

$$\|A\|_n = \|\alpha\| \times \begin{vmatrix} \frac{\nabla_n}{\nabla_{n-1}}, & 0, & 0, & \ldots & 0 \\ 0, & \frac{\nabla_{n-1}}{\nabla_{n-2}}, & 0, & \ldots & 0 \\ 0, & 0, & \frac{\nabla_{n-2}}{\nabla_{n-3}}, & \ldots & 0 \\ . & . & . & . & . \\ 0, & 0, & . & . & \frac{\nabla_1}{\nabla_0} \end{vmatrix} \times \|\beta\|."$$

The following result (among many which may be deduced from this transformation of a square matrix) admits of frequent applications:—

"If D be the greatest common divisor of the determinants of the matrix of any system of n independent linear equations; of the D^n sets of values (incongruous mod. D) that may be attributed to the absolute terms of the equations, the system is resoluble for D^{n-1}, and irresoluble for $D^{n-1}(D-1)$."

As an example of the use that may be made of this result, it is shown, in conclusion, that it supplies an immediate demonstration of a fundamental principle in the general theory of complex integral numbers, composed of the root of any irreducible equation, having its first coefficient unity, and all its coefficients integral; viz. that the number of incongruous residues, for any modulus, is always represented by the norm of the modulus. A demonstration of this principle has, however, already been given in the 'Quarterly Journal of Pure and Applied Mathematics,' in a paper signed *Lanavicensis*; to whom, therefore, the honour of priority in this inquiry is due.

"Contributions to the Physiology of the Liver—Influence of Alkalies." By Frederick W. Pavy, M.D,

Feb. 7.—The Bakerian Lecture.—On the Absorption and Radiation of Heat by Gases and Vapours, and on the Physical Connexion of Radiation, Absorption, and Conduction. By Professor Tyndall, F.R.S. (This paper was printed in full in the September and October Numbers of this Magazine.)

Feb. 14.—"On Magnetic Storms and Earth-Currents." By Charles V. Walker, Esq., F.R.S., F.R.A.S.

February 21.—Major-General Sabine, R.A., Treasurer and Vice-President, in the Chair.

The following communications were read:—

"On Terephthalic Acid and its derivatives." By Warren De la Rue, Ph.D., F.R.S. &c., and Hugo Müller, Ph.D., F.C.S.

Whilst pursuing our investigation of Burmese naphtha, an abstract of which we have already communicated to the Society, we noticed, among the products of the action of nitric acid on certain liquid hydrocarbons contained in Rangoon tar, an acid of peculiar properties. A very lengthened investigation of this acid and its de-

rivatives we are about bringing to a close; but as the drawing up of this account will necessarily occupy a considerable time, we have thought it desirable to send a short abstract of the chief results we have obtained, with the view of its appearing in the 'Proceedings' of the Society.

M. Caillot, about fifteen years ago, obtained a peculiar acid among the products of the action of dilute nitric acid on oil of turpentine, to which he gave the name of Terephthalic acid, on account of its generation from oil of turpentine and its isomerism with phthalic acid. M. Caillot's account of his new acid was so brief and incomplete, that, although we recognized many points of resemblance between it and the acid we had obtained from Burmese naphtha, we were compelled to repeat his experiments on oil of turpentine before we could fix with certainty the identity of the two products. In the course of these experiments, in which that identity was fully established, we noticed some interesting features in the compounds of the acid and the derivatives we discovered; more especially the relation of terephthalic acid to the well-known aromatic series,—a relation precisely analogous to that which succinic acid bears to the fatty acids. The close relation which exists between terephthalic acid and benzoic acid is most strikingly manifested in the great number of derivatives which are obtained from the former; indeed, nearly all of the most characteristic benzoyl-compounds have their analogues amongst the derivatives of terephthalic acid. Terephthalic acid being a bibasic acid, maintains its character throughout its various transformations, and it is this fact which claims particular interest.

Terephthalic acid, as well as its derivatives, forms the first term of a new series of well-characterized bodies, and may, as such, be considered the prototype of a great number of compounds still unknown.

Without dwelling at present on the tedious process by which terephthalic acid is produced, we may mention that it is obtainable from various sources. We have, for instance, found that it is invariably formed, in a relatively small proportion, when toluylic acid is prepared from cymole; it is also formed when cymole is treated with fuming nitric acid for the purpose of preparing nitrotoluylic acid. It is important to mention, that whether the cymole be prepared from oil of cumin or from camphor, the result is the same.

Subsequently, we found that insolinic acid, which was described some years ago by Hofmann as a new acid of the formula $C^{9}H^{4}O^{4}$, is in reality terephthalic acid. The formation of this acid from oil of cumin or cuminic aldehyde by the action of chromic acid on these substances, turned out to be the most ready method of preparing terephthalic acid; and the principal part of our experiments were made with terephthalic acid which had been obtained from oil of cumin by this process.

Terephthalic acid being isomeric with phthalic acid, has the formula $C^{8}H^{6}O^{4}$ (Carbon = 12, Oxygen = 16), as already known. When pure, it forms a white opake powder; but if thrown down from a boiling dilute alkaline solution, it may be obtained in a

crystalline state. When collected on a filter, these crystals dry in paper-like masses of a silky lustre. Terephthalic acid is not perceptibly soluble in ether, chloroform, acetic acid, water, or the other usual solvents. Concentrated sulphuric acid dissolves it to a considerable extent, especially when warm, without the formation of sulpho-terephthalic acid, and the acid separates unchanged on the addition of water. On heating, terephthalic acid sublimes without previously fusing. The sublimate, which is indistinctly crystalline, has the same composition and properties as the original acid, and therefore, unlike other bibasic acids, terephthalic acid cannot be converted into an anhydride by merely heating it. Terephthalic acid exhibits a remarkable deportment with regard to its salts; for although bibasic, there appear to exist no double salts; and even acid salts are only prepared with the greatest difficulty.

The alkaline terephthalates are all very soluble in water, but are insoluble in alcohol. The potassium, sodium, and ammonium compounds can be obtained in well-crystallized forms. The calcium and barium salts are less soluble than the before-named, and may be obtained in small scaly crystals. The copper salt is a pale blue crystalline powder. The silver and the lead salt occur as curdy precipitates when obtained by double decomposition. The compounds of terephthalic acid with the alcohol radicals possess a particular interest, as they furnish the most direct proof of the bibasic nature of the acid. There exist neutral and acid compounds. The neutral ethers are obtained either by the action of chloride of terephthalyle on the alcohols, or by means of the iodide of the alcohol radicals and terephthalate of silver or of potassium.

The methyl-terephthalic ether, $C^8 H^4 (CH^3)^2 O^4$, is the most characteristic compound, and consequently may be used to detect the existence of terephthalic acid in the presence of other acids. It forms beautiful flat prismatic crystals several inches long, which fuse at a temperature above 100° (Cent.), and sublime without decomposition. It is readily soluble in warm alcohol, and slightly soluble in cold alcohol.

The ethyl-terephthalic-ether forms long prismatic crystals resembling urea, and is readily soluble in cold alcohol.

The amyl-terephthalic-ether forms scaly crystals of pearly lustre, is readily soluble in alcohol, and fuses in the temperature of the hand.

Phenyl-terephthalic-ether, a white crystalline substance, fuses at above 100° C.

The acid compounds are generally formed in small quantities, along with the neutral ethers, by the action of the iodide of the alcohol radicals on terephthalate of silver. They are well-defined monobasic acids, and form crystallizable substances soluble in alcohol.

Nitro-terephthalic acid, $C^8 H^3 (NO^2) O^4$. This acid is formed by acting with a mixture of nitric and fuming sulphuric acid on terephthalic acid. When crystallized from certain solvents, it forms well-developed prismatic crystals of a faint yellow colour. From water, it deposits in cauliflower-like aggregations.

Nitro-terephthalic acid is readily soluble in warm alcohol and in

warm water, and possesses the bibasic character of the terephthalic acid in a much higher degree. It forms well-defined crystallizable acid and neutral salts. The ethers of this acid are likewise crystallizable. They differ, however, from the terephthalic acid ethers by their greater solubility in alcohol and their depressed fusing-point.

Chloride of terephthalyle ($C^8 H^4 O^2 Cl^2$) is obtained, together with oxychloride of phosphorus (hydrochloric acid being evolved), when terephthalic acid is acted upon with pentachloride of phosphorus at a temperature of 40° (Cent.). Chloride of terephthalyle is a solid and beautifully crystalline substance, without odour at the ordinary temperature, but evolving, when heated, a very pungent smell like that of chloride of benzoyle, which it resembles in all its reactions. With the alcohols it forms terephthalic ethers, with ammonia an amide, and with the organic bases compound amides. Terephthalylamide, $C^8 H^8 N^2 O^2$, can only be obtained by acting with chloride of terephthalyle on ammonia; it is a white amorphous substance insoluble in all solvents. Terephthalylamide, when treated with fuming nitric acid, yields nitro-terephthalic amide, $C^8 H^3 (NO^2) N^2 O^2$, which crystallizes in beautiful prisms.

Terephthalamide shows a remarkable resemblance to benzamide when treated with substances capable of abstracting the elements of water. It loses two equivalents of water ($H^2 O$), and is converted into terephthalylnitrile, $C^8 H^4 N^2$. This remarkable substance is best formed by the action of anhydrous phosphoric acid on terephthalamide. It distils over in form of a liquid, which solidifies in the neck of the retort.

Terephthalylnitrile is colourless and without odour, and forms beautiful prismatic crystals. It is insoluble in water, readily soluble in boiling alcohol, less soluble in cold alcohol, and insoluble in benzole. When boiled with caustic alkalies, it is gradually decomposed, ammonia is given off, and terephthalic acid is reproduced.

It is obvious that terephthalylnitrile, like all similar substances, may be considered as a cyanogen compound, which in this instance would be the cyanide of the bibasic radical phenylene, $C^6 H^4$, which is not yet discovered. If we could succeed in obtaining phenylene, the artificial production of terephthalic acid or an isomeric would probably be attended with little difficulty.

By acting on nitro-terephthalic acid with reducing agents, it undergoes the same change as other nitro-compounds. The product of this reaction is the oxy-terephthalamic acid, or the analogue of the glycocoll of the formula $C^8 H^7 NO^4$. This new member of the glycocolls is a lemon-yellow substance, crystallizing in thin prismatic, and sometimes moss-like forms. It is very slightly soluble in cold water, alcohol, ether, and chloroform. Like other substances of this kind, terephthal-glycocoll combines with bases as well as with acids. The salts formed with the bases are crystalline; they are readily soluble in water and dilute alcohol, yielding colourless solutions of most remarkable fluorescent properties, which have been investigated by Professor Stokes.

The aqueous and alcoholic solution of the pure terephthalic glycocoll

shows the same properties. The compounds with acids crystallize well, and if dissolved in a large quantity of water decompose. They do not possess the fluorescence when in their acid solution.

The ether-like compounds of oxy-terephthalamic acid are obtained by acting upon the corresponding ethers of the nitro-terephthalic acid with reducing agents. The methylic ether is a beautiful crystalline substance, readily soluble in warm alcohol, but much less soluble in any of the solvents than nitro-terephthalate of methyle. The ethylic ether crystallizes in large crystals with an appearance resembling those of nitrate of uranium. The solutions of this ether possess the fluorescent property in the highest degree. Oxy-terephthalamate of methyle and ethyle combine with acids and form well-defined salts. Oxy-terephthalamic acid, as well as its ether, are readily acted upon by nitrous acid, this reaction giving rise to a number of new derivatives, which vary in their nature according to the condition in which the reaction takes place.

M. Griess has lately made us acquainted with a new class of remarkable substances which are obtained by the action of nitrous acid on a certain class of nitrogenous bodies. The several derivatives he obtained by this reaction from oxy-benzamic acid have their representatives in the bibasic terephthalyle series, and are obtained with the utmost facility. On acting with nitrous acid upon an aqueous solution of the oxy-terephthalamic acid instead of an alcoholic solution, as is employed in Griess's reaction, this substance is readily decomposed, nitrogen is given off in large quantities, and there gradually separates a whitish substance which is oxy-terephthalic acid, $C^{8}H^{6}O^{5}$. This acid is a substance of great interest, and its preparation offering much less difficulty than the analogous oxy-acids of the aromatic series, it affords an opportunity of studying to a fuller extent the nature of this class of acids, especially as it may be expected that the history of this acid will throw some light on the law of polybasicity. Oxy-terephthalic acid forms beautiful crystalline salts, which are less soluble than the corresponding terephthalates. The neutral ethers are liquid.

The chloride of oxy-terephthalyle is likewise a liquid readily decomposed by water and alcohols.

"Notes on the Generative Organs, and on the Formation of the Egg in the Annulosa."—Part I. By John Lubbock, Esq., F.R.S.

February 28.—Major-General Sabine, R.A., Treasurer and Vice-President, in the Chair.

The following communications were read:—

"Tables of the Weights of the Human Body and the Internal Organs in the Sane and Insane of both Sexes at various Ages." By Robert Boyd, M.D., F.R.C.P.

"On the Electric Conducting Power of Copper and its Alloys." By A. Matthiessen, Ph.D.

The difference in the numerical results obtained by Prof. W. Thomson (Proceedings of Roy. Soc. 1860, x. p. 300), and those by Dr. Holzmann and myself (Phil. Trans. 1860), on the conducting power of copper and its alloys, made it somewhat necessary to re-

investigate the subject, in order to ascertain the cause of these differences. For this purpose Professor Thomson kindly placed at my disposal all his alloys; and in the following Table I will give the results of the analyses and redeterminations of the conducting power of his set. The wires were in some cases very faulty, so that I was obliged to draw them finer; others drew so badly, that the values obtained could not very well agree with those already published. After having measured their resistances, I sent them back to Prof. Thomson for redetermination. Table I. gives the results so obtained, taking the alloy containing 99·75 copper and ·25 silver=100; and Table II. the values found for some specimens of pure copper:—

TABLE I.

Composition according to Messrs. Johnson and Matthey.	Analyses of Alloy.	Specific Conductivity.		
		Values found by Professor Thomson.		Values found by myself.
		Published Values.	Redetermined Values.	
Copper 99·75..	Silver 0·24 p. c.	100	100·1	100·37 at 17°*
Silver 0·25....	traces of iron Suboxide of copper		99·9	99·73 at 17°
Copper 99·87..	Silver 0·13 p. c.	100·7	95·8	95·44 at 17°·8
Silver 0·13....	traces of iron Suboxide of copper		95·8	94·58 at 17°·8
Copper 99·75..	Lead 0·2 per cent.	103·9	102·7	102·80 at 17°
Lead 0·25	traces of iron Suboxide of copper		103·1	102·62 at 17°·6
Copper 99·75..	Tin 0·23 per cent.	94·6	100·7	99·89 at 18°
Tin 0·25.....	traces of iron Suboxide of copper		101·0	98·27 at 16°·4
Copper 99·87..	Tin 0·07 per cent.	96·0	97·7	97·79 at 18°
Tin 0·13.....	traces of iron. Suboxide of copper		98·5	97·62 at 18°
Copper 99·2 ..	Zinc, with traces of	90·2	91·3	94·71 at 15°·4
Zinc 0·8.....	iron, 1·06 per cent.		88·5	90·67 at 15°·6
Copper 98·6 ..	Zinc, with traces of	74·7	81·1	81·15 at 16°·8
Zinc 1·4.....	iron, 1·47 per cent.		80·1	80·13 at 17°·7
Copper 98·2 ..	Zinc, with traces of	..	77·9	77·8 at 16°·4
Zinc 1·8.....	iron, 1·75 per cent.		78·5	78·0 at 17°
Pure copper ..	Contained suboxide of copper	100	98·6	
Copper 99·87..	.. ..	104·7		
Lead 0·13	.. ..			

* Compared with a hard-drawn gold-silver wire of equal diameter and length, whose conducting power is equal at 0° C. to 100, these values would be 603·7 and 600·5. (See my paper "On an Alloy which may be used as a Standard of Electrical Resistance," Phil. Mag. Feb. 1861.)

TABLE II.

Composition according to Messrs. Johnson and Matthey.	Analyses of Alloy.	Specific Conductivity.		
		Values found by Professor Thomson.		Values found by myself.
		Published Values.	Redetermined Values.	
Pure copper electrotype from Messrs. De la Rue	All not fused.	.. ..	107 at 9°	107·2 at 10°
Ditto from Messrs. Elkington and Co. ..		.. ..	107·5 at 12°	105·9 at 10°·5
Ditto from Mr. Matthews		.. ..	108·7 at 12°	106·9 at 14°
Ditto, my own..	.. ..	.. ..	107·7 at 12°	108·1 at 10°

All the above wires were hard-drawn. On looking at the above, we find that *pure copper conducts better than any of the alloys.*

With regard to the analyses, the quantity of each specimen was so small that they could not be checked by repetition; they, however, approach very closely to the composition assigned to them by Messrs. Johnson and Matthey (with the exception of the suboxide). The traces of iron will be due to the draw-plates. I will now make a few remarks on the above results.

I. That copper containing 0·25 per cent. of silver conducts better than that with 0·13 per cent., may be explained by assuming that the first contains less suboxide than the second; for it is very possible that copper containing silver will not absorb suboxide so readily as the purer metal. It must also be borne in mind that the copper employed for making these alloys was in all probability simply electrotype copper (not fused), and that the suboxide therefore was absorbed during the process of fusing the two metals together. This assumption explains how it is that the alloys contain almost the same amount of impurity as was originally alloyed with the copper; for had the copper employed contained suboxide, we should have expected to have found greater differences in the cases of the tin, lead, and zinc alloys, as some portion of those metals would have been oxidized at the expense of part of the suboxide of copper, and escaped as oxide to the surface of the melted metal.

II. That copper containing 0·25 per cent. tin conducts better than that containing 0·13 per cent., may also be explained by assuming that they absorbed different amounts of suboxide during the process of fusion; for although tin, in presence of suboxide of copper, would be oxidized, yet copper retains the suboxide so tenaciously, that portions will always remain with the copper.

III. The fact that the conducting powers of the alloy of copper

containing 0·25 per cent. lead approaches the nearest of those which I analysed to that of pure copper, is, in my opinion, a proof that the alloy is probably a mechanical mixture of copper, traces of lead, and enough suboxide to allow its being drawn into wire, and not a solution of lead in copper ; otherwise a much lower conducting power ought to have been found ; for, according to my own experiments, it requires twice as many volumes per cent. of lead as of tin to reduce (within certain limits) the conducting power of a metal (bismuth, silver, &c., and copper, for it belongs to the same class) to the same value : thus, to reduce the conducting power of silver to 67, it would require 0·9 volume per cent. of lead, or about 0·4 volume per cent. of tin ; to reduce it to 47·6, it would require 1·4 volume per cent. of lead, or 0·7 volume per cent. of tin, &c. (Phil. Trans. 1860). Dr. Holzmann and myself repeatedly tried to draw pure copper alloyed with 0·25 of lead without success ; the alloy was perfectly rotten, which also seems to indicate a mechanical mixture.

IV. It is curious that the zinc alloys contained no suboxide.

The reason, therefore, of the difference in our results is simply that Messrs. Johnson and Matthey did not use those precautions in fusing their copper and its alloys which are necessary to ensure good results ; for had they taken those precautions to prevent the absorption of oxygen by their copper and its alloys which Dr. Holzmann and myself did, and which are fully described in our paper on the subject (Phil. Trans. 1860), the lead-copper alloys which they supplied to Prof. Thomson would not have been superior in conductive quality to the unalloyed electrotype copper ; and he would have been led to the same conclusion as that which Dr. Holzmann and myself arrived at, namely, *that there are no alloys of copper which conduct better than pure copper*. Professor Thomson, in his paper, states that it is his opinion that the differences he observed in the conducting powers of his alloys must depend upon very small admixtures of probably non-metallic impurities. This conclusion is completely borne out by the above, as well as by the investigation carried out by myself in conjunction with Dr. Holzmann.

The results obtained by Prof. Thomson show the marked influence of traces of foreign metals on the conducting power of pure copper,—which is fully confirmed in our research on the same subject. Professor Thomson's best-conducting alloy has a much higher conducting power than those found by some experimenters for electrotype copper ; but it must be remembered that in all probability the copper had been previously fused, and therefore contained suboxide of copper. The fact that electrotype copper may be drawn without having been previously fused is, I believe, generally not known ; Professor Buff of Giessen first drew my attention to it, and stated that he always obtained high values for the conducting powers of electrotype copper when drawn without previous fusion. I can confirm this statement, having tested a great many specimens, and found the values in all cases nearly the same.

March 7.—Major-General Sabine, R.A., Treasurer and Vice-President, in the Chair.

The following communications were read:—

"On Combustion in Rarefied Air." By Dr. Edward Frankland, F.R.S.

In the autumn of 1859, whilst accompanying Dr. Tyndall to the summit of Mont Blanc, I undertook at his request some experiments on the effect of atmospheric pressure upon the amount of combustible matter consumed by a common candle. I found that, taking the average of five experiments, a stearine candle diminished in weight 9·4 grammes when burnt for an hour at Chamounix; whilst its ignition for the same length of time on the summit of Mont Blanc, perfectly protected from currents of air, reduced its weight to the extent of 9·2 grammes.

This close approximation to the former number under such a widely different atmospheric pressure, goes far to prove that the rate of combustion is entirely independent of the density of the atmosphere.

It is impossible to repeat these determinations in a satisfactory manner with artificially rarefied atmospheres, owing to the heating of the apparatus which surrounds the candle, and the consequent guttering and unequal combustion of the latter; but an experiment in which a sperm candle was burnt first in air under a pressure of 28·7 inches of mercury, and then in air at 9 inches pressure, other conditions being as similar as possible in the two experiments, the consumption of sperm was found to be,—

At pressure of 28·7 inches 7·85 grms. of sperm per hour,
„ 9·0 „ 9·10 „ „

thus confirming, for higher degrees of rarefaction, the result previously obtained.

In burning the candles upon the summit of Mont Blanc, I was much struck by the comparatively small amount of light which they emitted. The lower and blue portion of the flame, which under ordinary circumstances scarcely rises to within a quarter of an inch of the apex of the wick, now extended to the height of $\frac{1}{2}$th of an inch above the cotton, thus greatly reducing the size of the luminous portion of the flame.

On returning to England, I repeated the experiments under circumstances which enabled me to ascertain, by photometrical measurements, the extent of this loss of illuminating effect in rarefied air. The results prove that a great reduction in the illuminating power of a candle ensues when the candle is transferred from air at the ordinary atmospheric pressure to rarefied air. It was, however, found that, owing to the circumstances mentioned above, no satisfactory quantitative experiments could be made with candles in artificially rarefied air, and recourse was therefore had to coal-gas, which, although also liable to certain disturbing influences, yet yielded results, during an extensive series of experiments, exhibiting sufficient uniformity to render them worthy of confidence. The gas was in all cases passed through a governor to secure uniformity of pressure in the delivery tubes. A single jet of gas was employed as the standard of comparison, and

this was fixed at one end of a Bunsen's photometer, whilst the flame to be submitted to various pressures, and which I will call the experimental flame, was placed at the other. The experimental flame was made to burn a uniform amount of gas, viz. 0·65 cubic foot per hour in all the experiments.

The products of combustion were completely removed, so that the experimental flame, which burnt with perfect steadiness, was always surrounded with pure air, the supply of which was, however, so regulated as to secure a maximum of illuminating effect in each observation.

In all the following series of experiments, the illuminating power given under each pressure is the average of twenty observations, which accord with each other very closely. In each series, the maximum illuminating effect, that is the light given by the experimental flame when burning under the full atmospheric pressure, is assumed to be 100. The following is a summary of the results:—

1st Series.

Pressure of air in inches of mercury.	Illuminating power of experimental flame.
29·9	100·
24·9	75·0
19·9	52·9
14·6	20·2
9·6	5·4
6·6	·9

2nd Series.

30·2	100·
28·2	91·4
26·2	80·6
24·2	73·0
22·2	61·4
20·2	47·8
18·2	37·4
16·2	29·4
14·2	19·8
12·2	12·5
10·2	3·6

These numbers indicate that even the natural oscillations of atmospheric pressure must produce a considerable variation in the amount of light emitted by gas-flames, and it was therefore important to determine, by a special series of observations, this variation in luminosity within, or nearly within, the usual fluctuations of the barometrical column. In order to attain greater delicacy in the pressure readings in these experiments, a water-gauge was used, but its indications are translated into inches of mercury in the following tabulated results, each of which represents, as before, the average of twenty observations.

3rd Series.

Pr. of air in in. of mercury.	Illum. power of exp. flame.
30·2	100·
29·2	95·0
28·2	89·7
27·2	84·4

It is thus evident that the combustion of an amount of gas which would give a light equal to 100 candles when the barometer stands at 31 inches, would give a light equal to only 84·4 candles if the barometer fell to 28 inches.

An inspection of all the above results shows that the rarefaction of air, from atmospheric pressure downwards, produces a uniformly diminishing illuminating power until the pressure is reduced to about 14 inches of mercury, below which the diminution of light proceeds at a less rapid rate. The above determinations give approximately 5·1 per cent. as the mean reduction of light for each diminution of 1 inch of mercurial pressure down to 14 inches. The following Table exhibits the actually observed light, compared with that calculated from this constant.

1st Series.

Pressure.	Illuminating power.	
	Observed.	Calculated.
29·9	100·	100·
24·9	75·0	74·5
19·9	52·9	49·0
14·6	20·2	22·0
9·6	5·4	— 3·5
6·6	·9	—18·8
2nd Series.		
30·2	100·	100·
28·2	91·4	89·8
26·2	80·6	79·6
24·2	73·0	69·4
22·2	61·4	59·2
20·2	47·8	49·0
18·2	37·4	38·8
16·2	29·4	28·6
14·2	19·8	18·4
12·2	12·5	8·2
10·2	3·6	— 2·0
3rd Series.		
30·2	100·	100·
29·2	95·0	94·9
28·2	89·7	89·8
27·2	84·4	84·7

I am now extending this inquiry to pressures exceeding that of the atmosphere, and hope soon to lay before the Society the detailed results of the whole series, together with some observations on the causes of this variation of luminosity.

"On the Porism of the In-and-circumscribed Polygon." By Arthur Cayley, Esq., F.R.S.

"On a New Auxiliary Equation in the Theory of Equations of the Fifth Order." By Arthur Cayley, Esq., F.R.S.

LXXI. *Intelligence and Miscellaneous Articles.*

NOTE ON THE FREEZING OF SALINE SOLUTIONS. BY M. RUDORFF.

WATER which contains saline substances in solution, freezes, as is well known, at a considerably lower temperature than pure water, and in passing into the solid state carries with it only a very small proportion of the dissolved salts. M. Rudorff proposed to investigate as large a number as possible of saline solutions, in reference to these two phenomena, and to ascertain if they were not subject to definite laws.

By experiments with solutions of common salt, of sulphate of copper, and of bichromate of potash, he first ascertained that the proportion of salt in the ice formed is always far less than that in the mother-liquor. The lamellar structure of the ice led him even to think that the salt which it contained was merely mechanically mixed, and that the ice, properly so called, was quite pure.

He then proceeded to a determination of the exact temperatures at which ice forms in different solutions. He prepared, with each of the salts, a series of solutions containing respectively 1, 2, 3 . . . . parts of salt to 100 of water. He placed each of them successively in a mixture of snow and salt, and stirred it with a thermometer which indicated twentieths of a degree Centigrade, until it solidified. Under these conditions, the freezing was sudden, simultaneous throughout all the mass, and accompanied by a marked increase of temperature. After this first determination (which was not entirely accurate, in consequence of the change in the composition of the liquor resulting from the instantaneous formation of a large quantity of ice) M. Rudorff made a second experiment, in which, without stirring the solution, he cooled it to about half a degree below the temperature at which the liquid froze in the preceding experiment. He then projected a few flakes of snow into the liquid, and observed that congelation took place accompanied by a slight increase of temperature, and he took the last indication of the thermometer for the freezing-point of the solution investigated. The formation of ice then continued for a long time without change of temperature; and it was only after a considerable quantity of ice had been formed that cooling commenced. If the solution was then taken out of the freezing-mixture and placed in a

room at a temperature of 12°, the thermometer rose to the temperature of the freezing-point, and remained almost constant until the whole of the ice was melted.

The salts suitable for these experiments ought to be very soluble at low temperatures, and to exert a marked influence on the freezing-point; they are hence few in number, and in fact are almost confined to the alkaline chlorides and nitrates. In most cases, M. Rudorff found that the lowering of the freezing-point below zero was proportional to the quantity of *anhydrous* salt dissolved in 100 parts of water. The following numbers give the lowering produced by the addition of 1 part of salt to 100 parts of water:—

Sal-ammoniac	0·653
Common salt	0·600
Chloride of potassium	0·443
Nitrate of ammonia	0·384
Nitrate of soda	0·370
Carbonate of potash	0·317
Nitrate of lime	0·277
Nitrate of potash	0·267

For chloride of calcium, the lowering of temperature appeared to be proportional to the quantity of salt crystallized with six equivalents of water, and not to the quantity of anhydrous salt which the solution contains. Designating by M the quantity of anhydrous salt, by M_1 the quantity of hydrated salt dissolved in 100 parts of water, by T the lowering of temperature, the experiments gave the following results:—

M.	M_1.	T.	$\frac{T}{M}$.	$\frac{T}{M_1}$.
2	4·02	0·90	0·450	0·224
4	8·21	1·85	0·462	0·225
8	17·20	3·90	0·487	0·225
14	31·89	7·40	0·528	0·232
18	43·05	10·00	0·555	0·232

We thus see that the value of $\frac{T}{M}$ increases somewhat rapidly with M, while that of $\frac{T}{M_1}$ is pretty constant.

At a temperature of −10°, crystals separate from a concentrated solution of common salt, which contain four equivalents of water for one of salt, and are rapidly destroyed by increase of temperature. This circumstance explains a remarkable result following from M. Rudorff's experiments. So long as the lowering of the freezing-point of a solution of common salt does not exceed 9°, it is proportional to the quantity of anhydrous salt dissolved. When it exceeds 9°, it becomes proportional to the quantity of salt crystallized with four equivalents of water. This will be seen from the

following Table, where the letters have the same meaning as in the Table relative to chloride of calcium :—

M.	M_1.	T.	$\frac{T}{M}$.	$\frac{T}{M_1}$.
1	.....	0°6	0·600	
2	.....	1·2	id.	
4	.....	2·4	id.	
6	.....	3·6	id.	
8	.....	4·8	id.	
10	.....	6·0	id.	
12	.....	7·2	id.	
14	.....	8·4	id.	
15	27·04	9·2	0·613	0·340
16	29·06	9·9	0·619	0·341
17	31·07	10·6	0·623	0·341
18	33 17	11·4	0·633	0·343
19	35·29	12·1	0·637	0·342
20	37·38	12·8	0·640	0·342

Lastly, the lowering of the freezing-point in the case of chloride of barium is proportional to the quantity of salt crystallized with two equivalents of water. The coefficient of proportionality is 0·192.—*Proceedings of the Academy of Sciences at Berlin*, April 1861.

EXPERIMENTS ON SOME AMALGAMS. BY J. P. JOULE, LL.D.

The weakness of the affinity which holds the constituents of amalgams in combination seemed to the author to offer the means of studying the relationship between chemical and mechanical force. His inquiries were extended to several amalgams, and gave results of which the following is a summary.

Amalgam of iron was formed by precipitating iron on mercury electrolytically. The solid amalgam containing the largest quantity of mercury appeared to be a binary compound. Iron does not appear to lose any of its magnetic virtue in consequence of its combination with mercury. Its amalgamation has the effect of making it negative with respect to iron in the electro-chemical series. The affinity between mercury and iron is so feeble that the amalgam is speedily decomposed when left undisturbed, and almost immediately when agitated. The application of a pressure of fifty tons to the square inch drives out so much mercury as to leave only 30 per cent. of it in the resulting button.

Amalgam of Copper.—By precipitating copper on mercury electrolytically, a mass of crystals is gradually formed. After a certain time the crystals begin to get fringed with pink, indicating uncombined copper. In this state the amalgam is found to be nearly a binary compound. On applying strong pressure to an amalgam containing excess of mercury, the latter is driven off, leaving a hard mass composed of equivalents of the metals. If, however, the pres-

sure be continued for a long time, the resulting amalgam contains more than one equivalent of copper, indicating a partial decomposition.

The author gave an account of his experiments with amalgams of silver, platinum, lead, zinc, and tin. In the case of the latter amalgam, long-continued pressure drives off nearly the whole of the mercury, indicating in a striking manner the efficacy of mechanical means to overcome feeble chemical affinities.—*From the Proceedings of the Literary and Philosophical Society of Manchester*, No. 8. Session 1861–62.

PRELIMINARY NOTE ON THE PRODUCTION OF VIBRATIONS AND MUSICAL SOUNDS BY ELECTROLYSIS. BY GEORGE GORE, ESQ.

If a large quantity of electricity is made to pass through a suitable good conducting electrolyte into a small surface of pure mercury, and especially if the mercurial surface is in the form of a narrow strip about $\frac{1}{8}$th of an inch wide, strong vibrations occur; and symmetrical crispations of singular beauty, accompanied by definite sounds, are produced at the mutual surfaces of the liquid metal and electrolyte.

In my experiments the crispations and sounds were readily produced by taking a circular pool of mercury from 1 to 3 inches in diameter, surrounded by a ring of mercury about $\frac{1}{8}$th or $\frac{1}{10}$th of an inch wide, both being contained in a circular vessel of glass or gutta percha, covering the liquid metal to a depth of about $\frac{1}{2}$ an inch with a rather strong aqueous solution of cyanide of potassium, connecting the pool of mercury by a platinum wire with the positive pole of a battery capable of forcing a rather large quantity of electricity through the liquid, and connecting the ring of mercury with the negative platinum wire. The ring of mercury immediately became covered with crispations or elevated sharp ridges about $\frac{1}{16}$th of an inch asunder, all radiating towards the centre of the vessel, and a definite or musical sound was produced capable of being heard, on some occasions, at a distance of about 40 or 50 feet. The vibrations and sounds ceased after a short time, but were always reproduced by reversing the direction of the electric current for a short time, and then restoring it to its original direction. The loudness of the sound depends greatly upon the power of the battery; if the battery was too strong the sounds did not occur. The battery I have used consists of 10 pairs of Smee's elements, each silver plate containing about 90 square inches of immersed or acting surface; and I have used with equal success six Grove's batteries, arranged either as 2 or 3 pairs, each platinum plate being 6 inches long and 4 inches wide. If the cyanide solution was too strong, the sounds were altogether prevented.

Being occupied in investigating the conditions and relations of this phenomenon with the intention of submitting a complete account of the results to the notice of the Royal Society, I refrain from stating further particulars on the present occasion.—*From the Proceedings of the Royal Society*, April 11, 1861.

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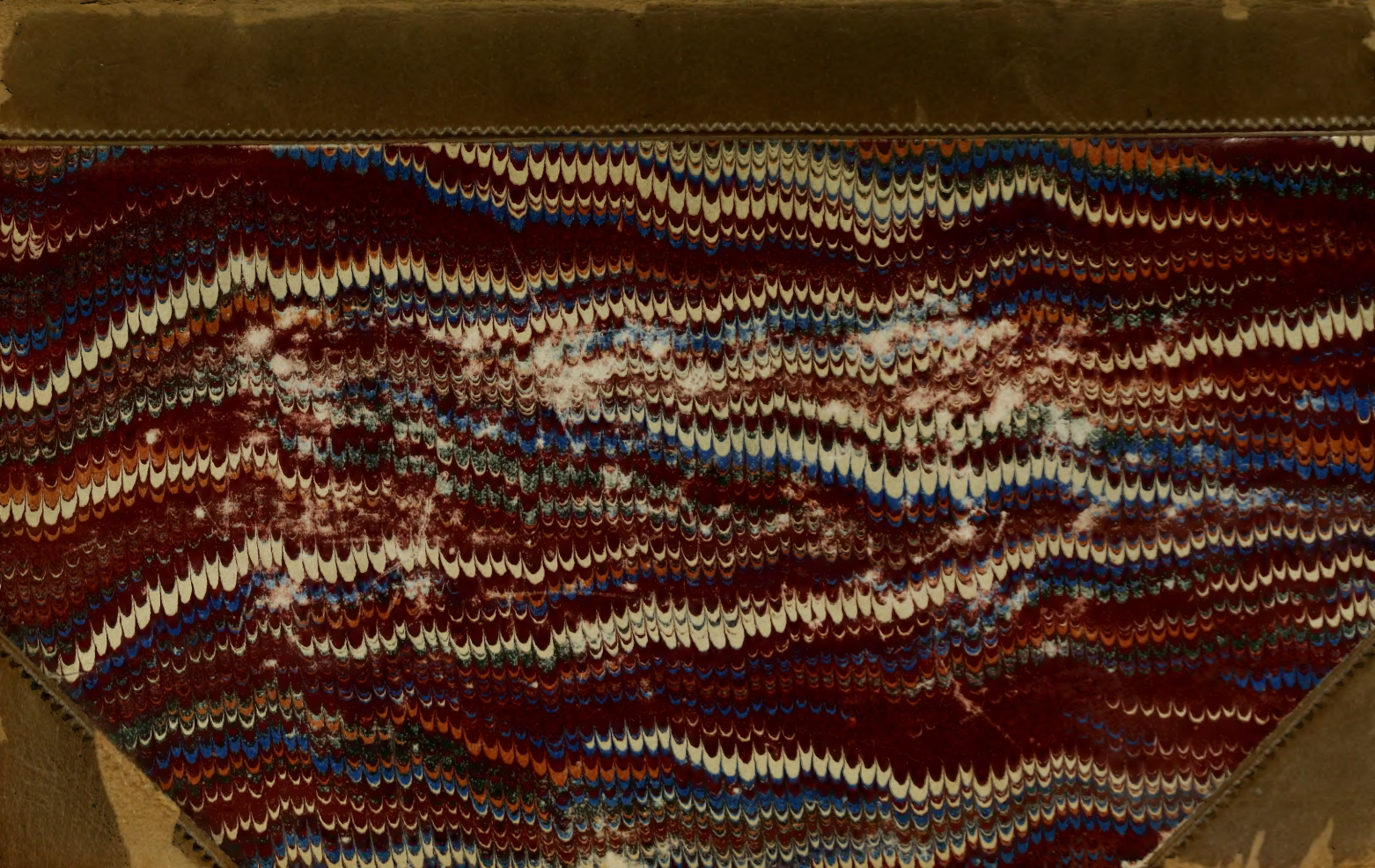
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